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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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TONNAGE LEGISLATION

WHEN it was announced, towards the close of the year 1880, that a Royal Commission had been appointed to consider the operation of the Tonnage Law, the action taken by the Government occasioned no surprise amongst persons interested in shipping. Disputes and differences of opinion, between the officials of the Board of Trade on the one side, and shipbuilders or shipowners on the other, had been growing more and more frequent in recent years; the rapid development of shipbuilding and the introduction of new types of ships or new systems of construction making difficult the application of the Law of 1854. When that Law was passed its language was clear and unmistakable, strictly applying to the ships then built. Wood still held the first place as the material for construction, and the technical terms used by Moorsom bore special reference to wood ships, although they were not inapplicable to the existing iron ships. Ships were then of moderate size and simple construction; ocean steam-navigation was comparatively in its infancy; and the marvellous growth in dimensions, speeds, and diversity of type which has taken place in the last quarter of a century could not have been foreseen—much less provided for in framing the Tonnage Law. It will readily be seen, therefore, that controversies of opinion were unavoidable when the Act of 1854 had to be extended to modern steamships, every clause being subjected to the closest scrutiny, and a strict legal interpretation being given to phrases which were originally clear enough, but of which the modern readings were doubtful or obscure. Shipowners naturally desired to secure the minimum nominal tonnage for their ships, since dues and taxes were assessed thereon; the Board of Trade surveyors, on the other hand, while acting with perfect fairness, might be expected to adopt an interpretation of the law which tended towards a tonnage exceeding that admitted by the owner. In some notable instances of recent occurrence the Board of Trade has either had to yield to these claims for reduced tonnage-measurement, or has been beaten in an appeal to the Law Courts; and it was natural, under these circumstances, that an attempt should be made to secure such an amendment of the Act of 1854 as was needed to prevent further controversy.

Nor was this the only reason for revision. Ever since the present tonnage law has been in force for British ships there has been a tendency on the part of other maritime nations to approximate to our system of measurement. The International arrangements made in connection with the Danube navigation and the Suez Canal have been based on the Moorsom system; and at the present time there is a closer approach to a uniform system of tonnage than has ever been reached before. This desirable result has been produced to a large extent by the action of the Board of Trade, whose successive Acts for the Amendment of the Law of 1854 have been adopted in foreign countries, although they have failed to secure Parliamentary approval at home. Consequently we stand, at present, in the curious position of still having in force the earliest and confessedly imperfect edition of the Moorsom system, whereas English experience and suggestion have given to other countries amended editions. On this ground, therefore, it was desirable to revise the tonnage law, even if the system remained unchanged in principle.

Further reasons for revision of a more thorough and sweeping kind were not wanting. It was admitted that the Law of 1854 was a great improvement upon its predecessors: more scientific in its mode of measurement, and having a sounder basis as applied to the ships built when it was framed, and to the then existing conditions of trade. On the other hand, it was alleged that subsequent changes in trade and shipping rendered the operation of the Tonnage Law injurious, hampering the skill of the shipbuilder, fostering certain inferior types of ships, and favouring heavy loading. In short, it was asserted that a change of system was needed on the grounds of greater safety to life and property, and greater fairness and freedom as between different types of ships.

All these reasons for inquiry are recognised in the Instructions issued to the Royal Commission. No one can fairly complain that the field of investigation is unduly limited; and a perusal of the evidence taken by, or the documents submitted to the Commission, will show that the exponents of every shade of opinion had the greatest

freedom allowed them in illustrating their views. If no other purpose had been served, the inquiry would have been justified by the very valuable summary of facts and opinions which it has been the means of putting on record. All that it is necessary to read for the full understanding of the past history of British tonnage laws can be found in the Minutes of Evidence or the Appendices; and much valuable information respecting foreign tonnage laws can also be found therein. Valuable as this feature of the work may be, however, it does not represent the purpose for which the Commission was primarily appointed, and when one turns to that aspect of the subject the results are not nearly so satisfactory.

In NATURE, vol. xxv. pp. 585-7, it was stated that the Commission did not make a unanimous Report. Three of its members dissented from the majority, and each of them produced a separate Report. This is not a matter for surprise; in fact it would not have been surprising had the Commission simply followed the precedent of the Parliamentary Committee of 1874, and submitted the evidence without making a Report. The majority consisted of nine gentlemen whose opinions are entitled to the greatest respect, including shipowners, shipbuilders, dock-proprietors, and representatives of the Board of Trade. As explained in vol. xxv. pp. 585-7, they recommend the retention of the principle of the existing law—viz. measurement of internal capacity—but propose certain amendments in detail. Some of these amendments are reasonable enough, but others are of questionable character; it is, however, of greater importance for our present purpose to consider whether the arguments advanced against a change in the principle of measurement are sufficiently weighty to prevent any change. Of all these arguments the most important are those relating to international obligation and convenience. Bearing in mind what was said above respecting the action of this country in leading up to a system of international tonnage, on the basis of internal measurement, it will be seen that an abandonment of that basis ought not to be lightly undertaken. But this fact need not bar the inquiry as to the advantages to be gained by such a change; for obviously the most thorough and exhaustive investigation would be needed, on other grounds, before the change could be made. And if after due investigation British shipowners were convinced that the change was desirable, the evidence which would convince them could scarcely fail to induce foreign maritime nations to follow our lead. The matter might well form the subject of an International Conference before final action was taken; much as was done in 1873 when the Suez Canal Regulations were framed.

Turning to the other side much more forcible arguments can be urged against a continuance of the present system. Moorsom took great pains to explain his reasons for using internal capacity as the basis of tonnage measurement; these may be summed up in the statement that internal capacity was the fairest measure of the possible earnings of most ships. This was probably the case in 1854; but is no longer true. In most ships the limit of freight-earning is now found in their "dead-weight capability"; that is to say, the prevalent cargoes of commerce do not now fill the whole space, when the weight taken on board has reached the limit of draught which can be accepted with a due regard to the safety of the ships. This is not true of all ships, but of most. Passenger ships, for instance, do not come under this condition; in them space is of the greatest value. Other types of ships, always engaged in carrying light cargoes still come under the condition which in 1854 was thought to be nearly universal. Still these cases are now the minority; and in the majority dead weight capability is the more important condition. This being so it is obviously unfair to assess the tonnage of all ships on the basis of internal capacity. In certain special classes, such as the "awning decked" class, it is alleged that the whole space available can never be filled with cargo; and on these classes the existing law bears heavily, although they are acknowledged to be eminently safe and seaworthy.

On other grounds the retention of internal capacity as the basis for tonnage is to be deprecated. Even with the amendments suggested in the Report of the Majority, there can be no hope that the disputes hitherto so frequent will cease, when a decision has to be arrived at respecting the spaces to be included in or excluded from the gross tonnage; and the deductions to be made therefrom in estimating the register tonnage. The Majority evidently realise this difficulty and attempt to meet it by more precise definitions; but the ingenuity which has been displayed in dealing with the phraseology of the Act of 1854 is not likely to fail in finding loopholes in the barriers now proposed. Nor can it be admitted that some of the proposals of the Majority are fair or foreseeing, having regard to the obvious tendencies and the possibilities of progress in shipbuilding and engineering. Into this discussion, however, it is not possible to enter here.

The three dissentient Reports contain much sharper criticism of the Majority Report than appears in the foregoing remarks. Exception has been taken to the tone of these Reports by some members of the Majority, but apparently not with much justice. Mr. Glover represents the shipowners who desire to be "let alone," and think the present Tonnage Law needs little or no change. Mr. Waymouth, accepting the view that "dead-weight capability" now rules the freight-earning of most ships, proposes to make dead-weight capability the basis of tonnage. Mr. Rothery advocates the "displacement," or total weight of the laden ship, as the fairest basis. Respecting Mr. Glover's views nothing need be said additional to what has already been stated respecting the working of the present system; but it is desirable to glance at the other proposals.

Mr. Waymouth has revived the oldest system of tonnage. "Keels" and other coal vessels were so measured time out of mind; and various empirical rules were framed for the "dead-weight capability" of other classes of ships. Being empirical they were easily evaded; Mr. Waymouth favours a well-known system of measurement, which takes account of the true form of the ship and renders evasion impossible. His system demands a fixed load-line; this is one of its difficulties. In order to overcome this objection to his scheme Mr. Waymouth favours the appointment of some central authority by which the load-line will be fixed. It is known that the Board of Trade is now taking preliminary action and ascertaining the

feeling of shipowners on the subject of establishing such a central authority. If it should be formed, then the load-line question might be dealt with more satisfactorily than it has been hitherto, and one difficulty in dead-weight measurement would disappear. But others, and probably fatal ones, would remain; more particularly in dealing with passenger steamers or vessels built to carry light cargoes. In such cases Mr. Waymouth proposes to fix, for tonnage purposes only, a deep load-line; this is not merely objectionable, but would probably be impracticable in many vessels. The dead-weight system has much to recommend it for consideration on the grounds of simplicity and exactness, as well as freedom from the difficulties incidental to internal measurement. But it is not likely to come into use.

Mr. Waymouth, it will be noted, agrees with the majority in proposing to continue the immemorial practice of basing tonnage measurement upon earnings or earning-power. This principle, although long accepted, has always been held open to question, on the ground that the accommodation provided for a ship in harbours, docks, canals, &c., should regulate the dues paid by her, and not her earnings. The "service rendered," and not the earnings, does appear the fairest basis of assessment, and has a considerable weight of authority to support it; but to adopt this basis would clearly necessitate a settlement of the mode of appraising service rendered. Mr. Rothery proposes to take the displacement, or volume of water displaced by a ship to a fixed load-line, as the measure of this service. The load-line, he suggests, might be fixed by the owner or some central authority. To this proposal many objections have been raised; but that which seems to have most force is found in the statement that the volume of water displaced does not measure the accommodation required, since various degrees of fineness of form under water might be associated with the same extreme dimensions—length, breadth, and draught. Two ships agreeing in these dimensions and requiring practically the same accommodation might differ in displacement by as much as 50 to 60 per cent. of the smaller.

Mr. Rothery's proposal has, however, done good in recalling attention to the principle of taxation on *service rendered*. In further investigations this is not likely to be overlooked; and it must be possible to frame some scheme which is not open to the objection to displacement above mentioned. The proposal to take the product of the three extreme dimensions of a ship as a basis for tonnage has been considered, and has much to recommend it, if associated with a fixed load-line. It cannot be said that any of these alternative schemes have received the full consideration they require before being brought forward for adoption. The investigation would necessarily be laborious, and the issues dependent upon it are so important that it should be intrusted only to competent and impartial hands. Certain conclusions are necessarily forced upon every person who makes a study of this subject. First, it is impossible in any revision of tonnage law to ignore the question of the load-line legislation. The majority of the Commission, in their final Report, propose to keep the two questions distinct; but it has been stated publicly by Mr. Waymouth that up to the very last draft Report, the majority made recommendations in the opposite direction; and if this is the case the less weight attaches to the recommendation which actually appears. Second: in considering future legislation, both for tonnage and for load-line, greater regard must be had to the provision of stability for merchant ships than has been had heretofore. Rough "rules of thumb" for free-board, in relation to depth of hold, are out of date. Third: the work to be done must be largely dependent upon the calculations made by competent naval architects for various types of ships, and various conditions of loading. Such calculations applied to vessels which have been thoroughly tested at sea under known conditions of lading must be the foundation for future rules for load-lines. Lastly, it is much to be desired that the proposed Shipping Council should be constituted, and that it should be a central body, including all classes interested in shipping, and having behind it a staff of skilled naval architects. The Marine Department of the Board of Trade has been much abused, and probably unfairly criticised in many cases. Its action, both as regards tonnage legislation and the load-line of ships, may not have been all that could be desired, yet it must be admitted to have been well intentioned. But it cannot be supposed that the Department as now constituted is capable of dealing with the questions pressing for solution. Neither its nautical, technical, nor adminstrative staff is competent for this task. And it may be supposed that the necessary reinforcement of that staff, the valuable assistance and advice of a Council of Shipping, and the more scientific investigation of matters relating to the safety and good behaviour of merchant ships by naval architects, will be welcomed by the Board of Trade as warmly as by the shipping community. Until these further investigations are completed, amended legislation scarcely seems practicable. It is clearly impossible on the lines laid down in the Report of the majority of the Royal Commission of 1881.

W. H. WHITE

MYTH AND SCIENCE

Myth and Science. An Essay. By Tito Vignoli. International Science Series. (London: Kegan Paul, Trench, and Co., 1882.)

THIS work is devoted to a theory of myths and myth-formation, which is to some extent novel. Looking to the general, if not universal, tendency of all races of mankind to create myths, the author contends that the propensity must point to some feature of human psychology of more than a merely superficial character, and without disputing previous theories as to the origin and growth of myths, he seeks to explain the *raison d'être* of the myth-forming faculty. Thus, for instance, he says:—

"The worship of the dead is undoubtedly one of the most abundant sources of myth, and Spencer, with his profound knowledge and keen discernment, was able to discuss the hypothesis as it deserves. . . . . Yet even if the truth of his doctrine should be in great measure proved, the question must still be asked how it happens that man vivifies and personifies his own image in duplicate, or else the apparitions of dreams or their reflections, and the echoes of nature, and ultimately the spirits of the dead."

And, speaking of Tylor, he adds:—

"He admits that there are in mankind various normal

and abnormal sources of myth, but he comes to the ultimate conclusion that they all depend on man's peculiar and spontaneous tendency to *animate* all things, whence his general principle has taken the name of *animism*. . . . . But, while assenting to this general principle, which remains as the sole ultimate source of all mythical representation, I repeat the usual inquiry; what causes man to animate all the objects which surrounds him, and what is the cause of this established and universal fact?"

And elsewhere the author states this problem thus:—

"To attain our object, it is necessary that the direct personification of natural phenomena, as well as the indirect personification of metaphor; the infusion of life into man's own shadow, into reflex images and dreams; the belief in the reality of normal illusions, as well as of the abnormal hallucinations of delirium, of madness, and of all forms of nervous affections; all these things must be resolved into a single generating act which explains and includes them."

Such being the problem with which the work is mainly concerned, its solution is attempted by the following theory:—Assuming the fundamental identity of human and brute psychology, it is argued *a priori* that, seeing the tendency to personify inanimate objects is so universal among primitive men, we might expect to find a similar tendency in animals, and this, according to the author, we do find:—

"Animals are accustomed to show such indifference towards numerous objects, that it might be supposed that they have an accurate conception of what is inanimate; but this arises from habit, from long experience, and partly also from the hereditary disposition of the organism towards this habit. But if the object should act in any unusual way, then the animating process which, as we have just said, was rendered static by its habitual exercise, again becomes dynamic, and the special and permanent character of the act is at once revealed."

And he proceeds to describe many experiments of his own, in frightening or surprising animals by making inanimate objects perform unusual movements. From these considerations and experiments he concludes that every object of perception is "implicity assumed" by an animal to be "a living, conscious, and acting subject;" that the animal transfuses into all things, "in proportion to the effects which result from them, his own nature, and modifies them in accordance with intrinsic form of his consciousness, his emotions, and his instincts."

This being taken as true of animals, the theory proceeds to the consideration that if we superimpose on the animal faculties of sensation and perception, the distinctively human faculties of reflection and symbolic thought, we should obtain a full explanation of the psychology of myth-formation.

We have said that this theory is to some extent novel, and it will now be seen that the extent to which it is so consists in its relegating to the domain of animal psychology that tendency to animism which has already been recognised as the feature in human psychology which is largely concerned in the formation of myth. But even thus far the theory is not wholly novel, for Comte supposed that animals possessed some crude ideas of fetishism, and Spencer, in his "Principles of Sociology," says:—

"Holding, as I have given reasons for doing, that fetishism is not original but derived, I cannot, of course coincide in this view; nevertheless I think the behaviour of intelligent animals elucidates the genesis of it;"

And he proceeds to detail cases which he has himself observed of "the idea of voluntary action being made nascent" in animals upon their seeing or feeling inanimate objects moving in unaccustomed ways. This, we think, is the whole extent to which the observed facts of animal intelligence entitle us to go. Uniformity of experience generates in animals, as in young children, organised knowledge of animate and inanimate objects, so that they are always more or less prepared with some antecedent expectation of the manner in which this or that object will behave. When, therefore, an inanimate object begins to move in some unaccustomed manner, the animal becomes alarmed, and no donbt "the idea of voluntary action becomes nascent."[1] But to argue from this fact that "every object, every phenomenon is for him a deliberating power, a living subject, in which consciousness and will act as they do in himself," and consequently that to animals the whole world "appears to be a vast and confused dramatic company, in which the subjects, with or without organic form, are always active, working in and through themselves, with benign or malignant, pleasing or hurtful influence"—to argue thus is surely to go far beyond anything that the facts either warrant or suggest. The very consideration that an animal shows alarm and horror when an inanimate object begins to behave like an animate one, points to the conclusion that he has made a pretty definite mental classification of objects as animate or inanimate. Therefore, without going further into the matter, it seems to us that the attempt made by this writer to argue for an universal animism as a feature of brute psychology, is a failure.

Of more interest and sounder theory is the part of his work which treats of the connection between Myth and Science. He says:—

Man, by means of his reduplicative faculty, retains a mental image of the personified subject, which is only transitory in the case of animals, and it thus becomes an inward fetish, by the same law, and consisting of the same elements, as that which is only extrinsic. These phantasms are, moreover, personified by the classifying process of types, they are transformed into human images, and arranged in hierarchy, and to this the various religions and mythologies of the world owe their origin. Since such a process is also the condition and form of knowledge, the source of myth and science is fundamentally the same, for they are generated by the same psychical fact"—*i.e.* that of ideally classifying objects of perception—"the historical source of the two great streams of the intellect, the mythical and the scientific, is found in the primitive act of *entifying* the phenomena presented to the senses"—in the one case with the conception of personality, and in the other with that of natural order.

This idea of myth and science having a common root in the rational faculty of man is not, of course, a profound one, seeing it is obvious that myth, like science, arises from the need or desire of reason to *explain* the facts of nature which are everywhere obtruded upon the observation of "the thinking animal"; but it is perhaps well that this truth should be clearly stated, as it is in the work before us. We think, however, that here, as indeed throughout, the work is needlessly protracted.

George J. Romanes

[1] See Nature, vol. xvii. p. 168 *et seq.*, where this subject is treated at more length.

A PRIMER OF ART

A Primer of Art. By John Collier. (London: Macmillan and Co., 1882.)

IN this admirable little work Mr. Collier has succeeded in bringing clearly into view the helpful relation in which science may stand to the Arts of Design—sculpture, drawing, and pre-eminently painting. The aim of the primer is to give the outlines of such knowledge of the artistic field of vision, of the visual powers, and of the means of delineation, as may best aid the student to acquire that power of strict imitation of natural objects which is the artist's first qualification.

The notion hitherto prevailing and perhaps somewhat superciliously held to on the part of art—that because the primary functions of science and of art respectively are widely different, therefore no legitimate help can be rendered by one to the other—is practically discredited in every page of Mr. Collier's little work. Throughout, his object is to pioneer the student to an artistic goal; throughout, the means employed have all the security of clear scientific principle. The theory of the Primer is that by knowing with scientific accuracy how some things *are*, the task of exhibiting artistically how other things *appear* may be greatly simplified.

After devoting a few charming pages to the latest suppositions concerning the origin of sculpture and drawing—pages illustrated by specimens of prehistoric and even palæolithic art—Mr. Collier quits "debateable ground" for that on which surer scientific light can be shed for the guidance of the student in the practice of art.

And here nothing is overlooked. Boundaries, Light and Shade, Texture, Perspective, Colour, and Contrast are the headings of so many terse and luminous little chapters, through each of which comes some word to the learner from the invisible world where science works, warning him how, unless he gives heed to certain hidden actualities within and without him, he may and probably will go many times wrong before he lights on the best way of rendering the natural objects before him.

Accurate seeing is necessary to ensure accurate delineation. The *facts* of simple appearance are what the art student needs to lay hold of. Science, whose constant business is with facts of every order, aids him here with suggestions how to discriminate between sight and inference—between that actual aspect of an object which is due to its present relation to the sight of the observer, and that compound mental view of it which is due to the mixed memory of many previous aspects. A perusal of Mr. Collier's pages on the nature of perspective, on the undulatory theory of light, on the action of a lens, on the structure and nervous mechanism of the eye, and on the physiological rationale of the phenomena of colour show how much scientific information can be given without the use of a single technical phrase.

Having learnt to see, the art student must further learn to delineate. Here again, in discussing the painter's media, it is still with the authority of science the teacher speaks. The chapter on "Turbid Media" clears up the difficulty respecting the varying behaviour of pigments as used on different "grounds." Here, too, as elsewhere, each practical suggestion is accompanied by a scientific reason why the means advocated should be adopted, such reason being always backed by some absolutely lucid explanation of the *nature* of the difficulty to be surmounted, or of the effect to be aimed at.

With the subject of landscape painting comes up the question of aërial perspective; and thereupon follow some admirable pages on the constitution of the atmosphere and the refraction of light. In dealing later with certain necessary discrepancies between natural appearances and their painted imitations, Mr. Collier clears out of the way, by a simple scientific consideration, an insidious problem with which the artistic beginner is apt needlessly to perplex himself—namely, how correctly to represent effects of light and shade within the very limited range of luminosity afforded by his materials. The solution lies within the sphere of optics. The eye takes next to no heed of the degree of total illumination; the *absolute* luminosity of the picture therefore does not signify. All that is needful is to render the *relative* proportions of light and shade in the object or scene depicted; the effect will then be accurate, since sight adapts itself readily and unconsciously to any *scale* of illumination that may be visible at one time.

For the rest, this little work of Mr. Collier possesses all the attributes of a first-rate primer. As we have observed, it is terse, clear, simple, instructive, and alluring. While the student receives aid from various departments of knowledge, calculated at once to forward his progress in painting, and to enrich his ideas of the world in which he works, there is nothing attempted to which the finished artist—aware as he is of the part played by imagination and by an incommunicable sense of harmony in the production of the finest art-work—can yet take any exception. Mr. Collier frankly admits the limitations of science with regard to these points, and leaves untouched all vexed questions concerning harmony of line and colour, on the ground that, important though they are, too little is known about them to make discussion profitable.

Yet that there is no real antagonism between accurate knowledge wherever it can be had, and the loftiest artistic imagination, and further, that science may help to free that imagination by giving it mastery over its means of expression, are truths borne witness to throughout the eighty-eight pages of the primer. The scientific reader will recognise in Mr. Collier's successful endeavour to link the rival sisters (Art and Science) in friendly partnership for the better portrayal of that Nature of which both are students, a welcome sign of the times, and an indication of the direction in which we may look for firmer ground than has hitherto been found for fruitful artistic discussion.

L. S. BEVINGTON

OUR BOOK SHELF

A Treatise on Rivers and Canals. By L. F. Vernon-Harcourt, M.A. Vol. I. Text, 352 pp.; Vol. II. Plates, 21 Pl. (Oxford: Clarendon Press, 1882.)

THIS work was intended (see Preface) to present "in a simple and concise form descriptions of the principal and most recent works on rivers and canals, and the principles on which they are based." It appears to have had its origin in a course of lectures delivered at the School of Military Engineering, Chatham, in 1880, but has been so carefully revised as to be free from the defects of a mere

reprint of a lecture course, and may be fairly said to fulfil well the object proposed in the preface. Great pains has evidently been taken to obtain data of actual examples of important works within the above scope; the series of twenty-one well-executed large plates of these is a most valuable feature of the work. The get-up of the work, being issued from the Clarendon Press, is of course excellent; the number of folds in the plates is an inconvenience (few have less than six, and one has ten folds), which might have been obviated by placing fewer diagrams on each plate. A very useful feature is the addition at the end of each chapter of a short summary of its matter, with many good practical remarks.

The work opens with a chapter on the physics of the subject, followed by one on discharge-measurement, then by one on general principles. Then come seven chapters on various appliances and details, viz., dredgers, piling, foundations, locks, inclines, lifts, fixed and movable weirs, dams, and movable bridges. Then follow one chapter on inland canals, one on great ship canals, one on protection from floods, four on improvement of tidal rivers, and lastly, one on the improvement of the mouths of tideless rivers.

From the great variety of subjects treated of in a compass of 322 pages, the treatment is sometimes unequal. The descriptions of the newest forms of the various appliances are, together with their illustrative plates, very interesting and instructive. But perhaps the most valuable part of the whole work is the last five chapters on the difficult and important subject of the improvement of river mouths; the few guiding principles that can be said to be known about so obscure a question are well brought out from the study of grand examples. The subject of discharge-measurement is not adequately treated: a reference to the recently-published (1881) "Roorkee Hydraulic Experiments" would probably have materially influenced this chapter in giving less importance to current-meters, and more to floats (especially tube-rods), and in the entire rejection of the old Chézy formula, $V=C\times\sqrt{RS}$, with a *constant* value of C. The chapter on inland canals is also (perhaps unavoidably) sketchy: thus the description of Indian canals covers only two pages, many of them being simply named.

ALLAN CUNNINGHAM

Galeni Pergamensis de Temperamentis et de Inæquali Intemperie Libri tres, Thoma Linacro Anglo Interprete, 1521. Reproduced in exact Facsimile. With an Introduction by Joseph Frank Payne, M.D. (Cambridge: Macmillan and Bowes.)

THE book before us is one of a series of facsimile reprints of eight books, published in the years 1521–22, by John Siberch, at the first press established at Cambridge; and it would appear, that after the issue of this series, no other works were published there until the year 1585, when a law was passed, limiting the printing of books to London and the Universities.

The revival of classical literature which swept over Europe towards the close of the fifteenth century, effected a complete revolution in the theory of medicine, as well as in philosophy. English scholars of that period were, as a rule, unacquainted with Greek, the few exceptions being men who had studied at the Italian Universities; among these was Thomas Linacre, a Fellow of All Souls, Oxford, who, about the year 1495, visited Italy in the suite of Selling, when the latter was appointed envoy to the Pope, and, after being a fellow student with the sons of Lorenzo de Medici, under Politiano and Chalcondylas, proceeded to his degree of Doctor of Medicine at Padua. On his return to England, he brought with him the reputation of being one of the most elegant and accurate scholars of the day. Shortly afterwards he was appointed tutor to Prince Arthur, and became Court physician on the accession of Henry VIII. to the throne. The physicians of the day were mostly ecclesiastics, but no restriction was placed on the practice of medicine by persons, however ignorant of its principles; and Linacre, with the view of remedying the abuses that prevailed, devoted his fortune, amassed by the sale of the clerical livings to which he had been presented, to the foundation, in the year 1518, of the Royal College of Physicians, which, under its charter, had power to regulate the practice of medicine in the neighbourhood of London. It is interesting to know, that according to Linacre, a physician should be "a grave and learned person, well read in Galen, respecting but not bowing down to the prestige of the Universities; claiming for his own science a dignity apart from, but not conflicting with that of theology; looking upon surgeons and apothecaries with charity, and not without a sense of his own superiority."

The Galenical theories of humours and temperaments formed the groundwork on which the Greeks based their practice of medicine, and Linacre to bring these theories within the reach of all students of medicine translated into Latin six of Galen's works, among which were the "De Temperamentis" and "De Inæquali Intemperie," now before us, thus helping to replace the mysticism and empiricism of the Arabians by the accumulated observations recorded by Hippocrates and Galen. In these works it is assumed that to the four humours, blood, pituite, yellow bile, and black bile, there are the corresponding properties, moist-heat, moist-cold, dry-heat, and dry-cold; and that between health and disease there are four temperaments, characterised by an excess of either one or two of the cardinal qualities, heat, cold, moisture, and dryness. These were the only external influences acting on the body the ancients could recognise, as they were ignorant of the chemical processes of respiration, of the constitution of the atmosphere, and of electricity, of which we now take account. These theories are elaborated, and further, it is indicated that medicines may be classified according to their heating, drying, cooling, or moistening qualities, and should be administered so as to temper the errors of the humours in disease; and though the work has ceased to have a practical value for physicians, it yet remains of interest to the student of humoral pathology, and of the philosophy of the middle ages.

Students are indebted to the enterprise of Messrs. Macmillan and Bowes for the reprint of this scarce work, which was the first book containing Greek characters printed in England, and we are glad to learn that the same publishers propose shortly to issue the remainder of the series. The book is edited by Dr. J. F. Payne, and is prefaced by a portrait and an admirable life of Linacre.

Rhopalocera Malayana: a Description of the Butterflies of the Malay Peninsula. By W. L. Distant. (London: W. L. Distant, care of West, Newman and Co., 54, Hatton Garden, E.C.)

WE have received the first part of this handsome work, in which it is proposed to describe and figure all the species of butterflies which inhabit the Malay Peninsula and the islands of Penang and Singapore. Forty-four coloured figures of butterflies are given in this part, occupying four plates of large quarto size: and they are most admirably executed in chromo-lithography. Some of the figures, indeed, are hardly to be distinguished from good hand-colouring. The descriptions are full and careful, and much judgment is shown in using, as far as possible, old and well-established names, and in rejecting needless sub-divisions of the genera. It is expected that the work will be completed in six or seven parts, forming a handsome quarto volume; and we trust that the author may obtain numerous subscribers in our wealthy colonies of Singapore and Penang, as well as at home, to encourage him to complete the work in the same full and careful manner as he has commenced it.

As most of the butterflies of the larger Malay Islands

must be studied in comparison with those of the Malay Peninsula for the purposes of his work, we would suggest to Mr. Distant that he would add greatly to its value to all European collectors if he would give, in a supplementary part, a complete synopsis of the known species of butterflies inhabiting the Indo-Malayan region. Having figured all the continental Malayan species, the descriptions of those of the islands might be, in most cases, by comparative characters, aided occasionally, perhaps, by outline woodcuts. We believe that such an extension of the scope of the work would double its value, and add largely to the list of subscribers; while the increased expenditure would be comparatively unimportant.

A. R. W.

Conic Sections Treated Geometrically. By S. H. Haslam, M.A., and J. Edwards, M.A. (London: Longmans, 1881.)

THIS is a neat little treatise on the conic sections, containing—what appears to be a novelty—a method of *plane projection*, to which the authors give the name of *Focal Projection*. The remarkable feature of the book is, that the authors, who are evidently well up in these curves, should not be acquainted with the writings of the present master of St. John's College, on the same subject. No one who has looked into Dr. Taylor's recent works, could be unacquainted with what he has said upon the contributions of Boscovich and G. Walker, and would hardly use the "generating circle" of a conic in the same fashion as Boscovich does, and write, after the definition, "which we have called the *auxiliary circle of a point*."

Schwatka's Search: Sledging in the Arctic in Quest of the Franklin Records. By W. H. Gilder. Maps and Illustrations. (London: Sampson Low and Co.)

THIS is the complete record of the expedition sent out by private subscription, in 1878, under Lieut. Schwatka, to endeavour to find the records of the Franklin expedition, which were reported to be in possession of the Nechelli Eskimo. With the general results of the expedition, our readers have already been made acquainted. The reported records, as might have been expected, were never found. But in and around the Fish River, and in King William Land and neighbourhood, several relics were obtained, and several graves and cairns found. The expedition, indeed, completed the story of the sad disaster of the *Erebus* and *Terror*. During the search, sledge-journeys of upwards of 3000 miles were made, and thus much welcome additional information was obtained concerning the country between Hudson's Bay and King William Sound. The expedition came a good deal into contact with the Eskimo, concerning whom Mr. Gilder has much to tell us. The narrative is interesting, and is welcome as throwing additional light on an Arctic expedition in which Englishmen have always continued to be interested. There are a number of good illustrations.

Chambers's Etymological Dictionary of the English Language. A new and thoroughly revised edition. Edited by Andrew Findlater, M.A., LL.D. (Edinburgh: W. and R. Chambers, 1882.)

THIS little work, since the publication of the first edition, edited by the late Mr. James Donald, has had deservedly a very extensive circulation. It is just the book to have at one's elbow for constant reference, handy, clearly printed, fairly full, and thoroughly trustworthy. This new edition has evidently been so thoroughly revised by Dr. Findlater, as to be virtually a new work. The selection of words has been made with great discrimination, the definitions are clear and comprehensive, and the etymologies up to the latest results of linguistic research. The dictionary contains a large number of scientific terms, though there are one or two others that we think ought to have found a place. The dictionary is the best of its class; the appendix contains much useful information, including a table of the Metric System.

Tunis; The Land and the People. By the Chevalier de Hesse-Wartegg. (London: Chatto and Windus, 1882.)

HERR VON HESSE-WARTEGG spent some months in Tunis last year, and has made a readable book out of his notes. He has also drawn largely on other sources of information, so that those who know little about a country which has been so much before the public recently, will find some useful information in this volume. The author spent a good deal of time about Tunis and its environs, but seems also to have visited several other places in the Regency, including, apparently, Kairwan. He tells us a good deal about the people and their customs, about the government, the Bishas, antiquities, &c. There are several good illustrations, but no map.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Silurian Fossils in the North-west Highlands

ON my return to-day from a geological tour in the North-west Highlands, I read with much interest the letters of Mr. Hudleston and Prof. Bonney on the above subject. The question as to the geological position of the limestone series in West Ross-shire, correlated by Murchison with the Durness limestone, is one to which I devoted special attention during my late visit to the neighbourhoods of Lochs Carron, Doule, Kishorn, and Maree. The general appearance of the limestone in these areas is sufficiently like that of Durness to lead to the supposition that it is of the same age, but this of course can only be proved by fossil evidence. The conclusion at which I have arrived, however, in regard to the geological position of the limestone, and as to its relation to the so-called Upper Gneiss rocks of the central parts of Ross-shire, is in direct opposition to the views of Murchison, and accords in the main with that of Prof. Nicol. The great triangular patch at the head of Loch Kishorn consists of a series of thick beds of grey limestone, with a few bands of sandstone in an unaltered condition, and is undoubtedly dropped by faults amongst much older strata, as maintained by Prof. Nicol. Between Lochs Carron and Doule the same series is seen to rest unconformably upon much higher beds than those which it is supposed by Murchison and others to underlie at Loch Kishorn. As I purpose shortly to give a full account of these researches, I will not venture now to trespass further on your space.

HENRY HICKS

Hendon, N.W., April 29

Earthquakes and Mountain Ranges

IN NATURE of the 27th ult., in a note on a recent meeting of the Seismological Society of Japan, it is mentioned that the observations of Prof. Milne "as far as they have at present gone, show in a remarkable manner how a large mountain range absorbs earthquake energy." It may be worth while to mention, as an exception to this, that the Swiss earthquake at 1 p.m. on July 25, 1855, which apparently had its origin among the mountains on the south side of the Valais, between Visp and S. Nicholas, both of which places were seriously damaged, travelled through the Bernese Oberland, across the great valley of Switzerland, and then through the Jura. I was at the time in a small inn, at a place called Belle Rive in the Munster Thal, on the north side of the Jura. The house was severely shaken, so that some plaster fell from the ceiling. This was about seventy-five miles from the place of origin, and the wave in that interval had passed through two mountain ranges. It is probable that this earthquake was caused by a disturbance of a quite different kind from the volcanic disturbances of Japan, and that may account for a difference in the result.

O. FISHER

Vivisection

THE editor of NATURE has no room for proofs; I must, therefore, confine myself to statements of facts which "A Student of Medicine" can verify by consulting the books I shall name.

Prof. Schiff is the author of several works detailing an enormous number of vivisections. Some six years ago the "Gazetta d'Italia" calculated that, of dogs alone, he had used in his laboratory 14,000—supplied to him gratuitously by the municipality of Florence—besides great numbers of other animals otherwise procured. Afterwards he received only about eight dogs a week from the police, and, in consequence, posted up an advertisement offering a franc for every dog brought to him, and a *bonus* upon ten dogs brought by one person. In the "Physiologie de la Digestion" he says: "I am forced to cut the vocal cords of the greater number of my dogs, lest their nightly howlings should compromise my physiological pursuits."

In Prof. Schiff's "Lezioni di Fisiologia Sperimentale" will be found ample justification of my statements with regard to the character of the vivisections performed by him. I regret much that there is no room to quote examples here. It will be observed by the reader of these "Lessons of experimental physiology," that the nature and duration of the majority of the experiments render the administration of chloroform either impossible, or at the best, utterly inadequate to hinder suffering. The animal is either *distinctly sensible* throughout the experiment, or has been already operated on some days previously, being preserved in a mangled and paralysed condition for further experiment.

As regards the "anæsthesia," I have to-day questioned a medical graduate, who resided seven years in Florence while Prof. Schiff lived there, and was constantly in his laboratory. He says that, although chloroform was commonly administered on tying down the animals (he believes, for the convenience of the operator), no pretence was made of keeping them under the influence of the anæsthetic after the preliminary incision, and that—as in fact is conclusively proved in Prof. Schiff's own works—mutilated animals were reserved from day to day, and from week to week, for further investigation.

If since leaving Florence and publishing his "Lessons" Prof. Schiff has mended his ways, I am sincerely glad to hear it. Should such, indeed, prove to be the case, public opinion at Florence—which ran high against him and his doings—must be credited with some share in the reformation.

In conclusion, let me say in regard to the discourteous charges of "error," "ignorance," and disregard of "facts" so freely brought against me by your correspondent, that if in this case any person is amenable to them, it is certainly not one who, being perfectly acquainted with the works both of Prof. Mantegazza and of Prof. Schiff, bases her estimate of their performances on *their own explicit statements.* ANNA KINGSFORD

11, Chapel Street, Park Lane, W., April 14

[MRS. KINGSFORD's letter was forwarded to our correspondent in Geneva, who sends the following reply:—]

In reply to Mrs. Kingsford's letter, I have the following remarks to offer:—

1. If Mrs. Kingsford is "perfectly acquainted" with the works of Prof. Schiff, she must know that there exists a small book in which he has explained in detail his methods of vivisection, &c. The title is: "Sofra il metodo seguito negli esperimenti sugli animali viventi nel laoratorio di fisiologia di Firenze." It first appeared in 1864; the second enlarged edition bears the date 1874. The book is written in a popular style, so as to render it easy even for the uninitiated to understand what vivisection is, and how it is practised by Prof. Schiff; it should be read by every person desirous of forming an unbiased judgment on the subject.

2. My letter was a reply to the accusation brought against Prof. Schiff of perpetrating "horrible tortures," "atrocities," &c. The *number* of dogs used for experiments (which number is erroneously stated), and the price supposed to have been paid for them are evidently irrelevant to the question. Moreover, you will find stated on p. 53 of the above pamphlet, that Prof. Schiff never accepted a dog unless its owner assured him that he would otherwise kill it himself; and I can corroborate from personal recent experience here in Geneva the further statement in the same passage, that if a person likely to be a kind master offers to take one of his dogs, he is always ready to give it away.

3. I regret to find that Mrs. Kingsford allows herself to misquote. The passage in the "Physiologie de la Digestion" referred to is on p. 291 of vol. i.: "Je suis *obligé* de faire subir la section des *nerfs laryngés à beaucoup* de nos chiens." The section of the vocal cords is a dreadful operation, that of the nerves in question so slight, that if performed on dogs whilst at their meals, they do not leave off eating! To this I can testify. Moreover, you will notice that Schiff says, "Je suis *obligé*," and the fact is he was *forced by the police* in Florence to cut the nerves in question; not indeed on most of his dogs, but on those which were not used for vivisection properly so called, but were kept during long periods for other (and perfectly painless) observations, such as those detailed regarding the very dog mentioned in that lecture (see the 13th lesson, p. 325).

4. People not versed in physiology are not competent to draw conclusions from a work such as the "Lezioni di Fisiologia sperimentale." Mrs. Kingsford herself offers an example of how gross the errors are into which they may fall when she declares that in the majority of experiments the administration of anæsthetics is either "impossible or inadequate." On p. 70 of the pamphlet "Sofra il metodo," &c., Schiff says: "Nello stato attuale delle nostre conoscenza non existo *un solo esperimento* praticato nell' animale viventi, al qualo non si possa, e quindi non si debba, togliere il carattere di crudeltà mediante l'uso degli anestetici;" and lower down: "Da 25 anni non mi sono *neppure una volta* veduto nella necessità di escludere l'uso degli anestetici." On p. 52 he writes: "Brediamo dover aspettare finchè ogni traccia di sensibilità, e l'effetto meccanico delle sensazioni sia scomparso."

5. To the medical graduate's statement and insinuations I oppose Prof. Schiff's affirmation and my own knowledge of his character and scientific habits. It is curious that this gentleman, after having spent seven years in Prof Schiff's laboratory, should be ignorant that *chloroform* is *never* used by him (see p. 49).

6. That many animals which have been operated upon are kept alive for ulterior observations is expressly stated in my first letter, and any person, however ignorant of science, can understand that whole branches of physiology can only be studied under the condition of this being so. They are kept not only as Mrs. Kingsford so pathetically exclaims, "from day to day, and from week to week," but sometimes from year to year." The question at issue is whether they suffer or not, a question easy to decide by their appearance, appetite, and demeanour. And the fact is they *do not suffer*, a statement any one can corroborate who chooses to come and look at the dogs in the School of Medicine here. *Why* they do not suffer is explained in detail in the pamphlet referred to above.

7. Prof. Schiff has *not*, alas, "mended his ways in deference to public opinion;" he tells me that never since the year 1847 has he departed from the methods detailed in the book quoted at the beginning of this letter.

Geneva, April 23 B.SC., STUDENT OF MEDICINE

Red Variable Stars—"Variab. Cygni (Birmingham), 1881," &c.

THE above star, so called by Schmidt in the *Astr. Nach.*, No. 2421, is now a striking object of 8 magnitude. On December 21, last year, it was certainly not over 12, and, probably, it was less. This appeared about its minimum, and its maximum seemed to have been attained on June 6, when it was 8 mag., as at present. On May 22, when I first found it, it was about 9. If it is now at maximum, there must be a striking inequality in its periods of decrease and increase, but perhaps it will go on to a greater magnitude this time than before.

U Cygni (No. 553 in my Red Star Catalogue) seemed last night (April 28) to be smaller than I ever saw it previously, and under 11 mag. Its colour was, however, very marked. The blue star near it (Arg. $+47^\circ$ 3078), which I have long considered to be slightly variable (see Catalogue), seems now at a maximum of 8 magnitude, though contrast with its diminished neighbour, may have some effect on its apparent size.

No. 448, in which I have also detected variability, is now about 8·5, and as deeply coloured as when I first found it in April, 1876. J. BIRMINGHAM

Millbrook, Tuam, April 29

Matter and Magneto-electric Action

THE very interesting lecture by Mr. Spottiswoode on the above subject incidentally throws light upon a phenomenon which probably has puzzled some other of your readers besides myself.

When a somewhat weak current is passing between the knobs of a Becker-Voss electro-induction machine, its passage can be altogether stopped by simply blowing across the path of the current. The handle is turned in vain; and even when the blowing has ceased, a short time is required before the current is able to pursue its old path. When the instrument has been warmed, and the current becomes stronger, the blowing, although now unable to stop the current altogether, drives it into irregularly curved paths, which are determined by the force exerted. I do not remember to have seen the experiment mentioned in any book. It is as curious as it is simple.

We now see why the air requires to be at rest for the weak current to force a passage through it, and to keep that passage open for the succeeding sparks to follow; while the stronger current leaps from point to point, as though in pursuit of the warmed and opened passage which has been driven by the wind out of its former position.

HENRY BEDFORD

All Hallow's College, Dublin, April 15

CYCLONES[1]

SINCE it first became known that a considerable proportion of the storms which visit this part of Europe come from the middle and northern parts of North America, the meteorology of that country has been invested with a peculiar and increasing interest for the inhabitants of Western Europe, and though, according to Hoffmeyer, the chance that a depression in the United States will subsequently cause a storm somewhere in our own islands is only one in four, it is a ratio quite substantial enough to make us regard with attention warnings such as those transmitted to us through the medium of the *New York Herald*.

While America is thus from her enormous size and westerly position enabled to act the part of our weather prophetess, she bids fair in addition to leave us far behind in the more theoretical branches of weather-science, and though to admit this may be somewhat wounding to our national *amour propre*, it is nevertheless an idea which is openly entertained by some of our leading meteorologists. For our comfort it may be reasonably ascribed, in part at least, to our small size and unfavourable geographical position having afforded but little encouragement to really able men to devote their attention to a science whose operations are conducted on a scale compared with which our area of observation is indeed microscopic, so that until within quite recent times the succession of fair and foul weather in these islands was regarded merely as a series of irregular, eccentric, and totally unpredictable changes. The work before us, entitled "Methods and Results," by Prof. William Ferrel, of the American Coast Survey, and prepared for the use of the coast pilot, forms the second part of a series of meteorological researches undertaken by the author, which comprise an elaborate theoretical investigation into the general and local mechanics of the atmosphere. In Part I., which appeared in 1877, the general motions of the atmosphere are more particularly dealt with, and conclusions are arrived at which have appeared in part in the *Mathematical Monthly* for 1860 and the *American Journal* for November, 1874.[2]

In both these publications the author lays great stress upon the important part played by the deflecting force to the right of its path, to which a current of air is subjected by virtue of the earth's rotation *in whatever direction it may be blowing*. This deflecting force is measured by the acceleration $2n \cos \psi$, where n represents the angular velocity of terrestrial rotation, and ψ is the co-latitude (see NATURE, vol. v. p. 384).

With the assistance of this element he theoretically deduces in Part I. the general motions of the atmosphere, which agree with what is known from observation. He also makes considerable use of this same principle, which he was the first to enunciate correctly, when dealing with the theory of cyclones in Part II. As we propose just now to confine our attention to Part II., which treats mainly of cyclones, we shall not refer to Part I. except incidentally. Part II. is sub-divided into three chapters, the first of which deals with the mechanical theory of cyclones, and deductions therefrom. In Chapter II. the results of the theory are compared with those of observation, and Chapter III. treats of tornadoes, hailstorms, and waterspouts. The chief elements considered in the theory of cyclones are (1) the earth's rotation, (2) the gyratory velocity round the low centre, (3) the friction, (4) the inertia, and (5) the temperature and humidity of the air.

These elements are all discussed in turn, and many important conclusions drawn from the resulting equations. Some of these conclusions have already been either directly deduced by the employment of analogous methods, or inductively inferred from an examination of data, by Guldberg and Mohn, Colding, Peslin, Sprung,[1] Clement Ley, Hildebrandsson, Meldrum, Loomis, and Toynbee. Some however are quite new, especially those which are derived from a consideration of the temperature term.

The general theory of the cyclone, according to Ferrel, may be briefly stated thus:—

If from any initial cause interchanging motions are set up between the air in a certain district and another surrounding it, the air in the first district tends to gyrate round its centre by virtue of the deflective force of the earth's rotation, and in the same direction as that of the component of terrestrial rotation, which acts in the plane of its horizon. In the northern hemisphere this would mean gyration contrary to watch-hands, and in the southern hemisphere gyration with watch-hands. In the outer district the gyrations of the air, by the principle of the preservation of areas (or moments), are contrary to those of the interior district. These two systems of contrary gyrations tend to draw the air from the centre of the inner district and the exterior limit of the outer district, and heap it up in the place where the gyratory velocity vanishes and changes sign, thus causing a maximum barometric pressure there, with corresponding minima at the centre and outer limit respectively.

In addition to this, when the gyrations have once commenced they give rise to a centrifugal force which tends to drive the air still more from the centre of the inner district, and so increase the barometric depression there; but which in the outer district, partly owing to its distance from the centre, and partly to the small velocity of the gyrations, has but little effect on the distribution of pressure. The gyrations, especially near the centre and exterior limit, would be very rapid, were it not for the friction between the air and the earth's surface, which retards the motion, but does not entirely prevent it, since, as the author very pointedly remarks, "without some such motion frictional resistance would not be brought into action." So far we have only considered the gyratory component of motion, and as in the imaginary case of no friction, this would be the only kind of motion, the gyrations might then be entirely circular. When, however, as actually happens in the atmosphere, friction acts, a radical component becomes necessary, since the deflecting force is now partly employed in counteracting the frictional resistance to the gyrations, and the magnitude of this radial component (on which depends the inclination of the wind to the isobar), varies *cœteris paribus* directly with the amount of friction.[2] As a result of the two

[1] "Methods and Results of Meteorological Researches for the use of the Coast Pilot." Part II.—On Cyclones, Waterspouts, and Tornadoes. By William Ferrel. (Washington, 1880.)

[2] "Relation between the Barometric Gradient and the Velocity of the Wind," by W. Ferrel, Assistant U.S. Coast Survey.

[1] "Die Trägheits-curven auf rotirenden Oberflächen," *Zeitschrift für Meteorologie*, Band xv., January Heft, 1880.

[2] This result is best seen in the following expression for the angle of inclination of the wind to the isobar $\tan i = \frac{f}{2\left(n \cos \psi + \frac{s \cos i}{r}\right)}$, where f is the coefficient of friction, s the velocity of the wind, and r the dis from the low centre.

tendencies—gyration and inflow, or outflow according as the air is in the interior or exterior part—the air near the surface takes a middle course, and flows spirally around and toward the centre from the zone of maximum pressure on the one side, and on the other in a contrary spiral outwards from the centre to the outer limit of the anticyclone.

It is important to observe that the author explains the accumulation of air with its maximum at the dividing line between the interior and exterior districts (cyclone and anticyclone, as they are termed elsewhere throughout this work) as "due at the start mostly to the gyrations in the *upper* part of the atmosphere," which, being less influenced by friction, are in consequence more circular than those below; the pressure from this accumulation tending to force the air near the earth's surface out from beneath it on the one side toward the centre of the cyclone, and on the other toward the outer limit of the anticyclone.

The difference of pressures or gradient between the regions of high and low pressure in a cyclone, is thus shown to be, not so much the *cause* of the wind, as the mechanical result of the deflecting force of the earth's rotation and the centrifugal force engendered by the gyrations.

It should, however, be borne in mind, that the forces just mentioned, are by no means to be regarded as causing the cyclone in the sense of being *independent* sources of energy. They can only arise in consequence of some initial motion of the air, which must itself be due to a small difference of pressure, and unless such primary disturbing cause be continually maintained by external influences, the entire system of motion will shortly come to rest.

The preceding view of cyclone generation has already made some way since Ferrel first enunciated its leading characteristics in his previous writings. It lies midway between what is sometimes called the in-blowing or ascension-current theory of Reye and Espy, which regards the central depression as the main cause of the wind, and that held by Thom, Meldrum, Willson, and Loomis, according to which the central depression is mainly due to the centrifugal force generated by two pre-existing currents passing one another in opposite directions. A third theory, held by Blanford and Eliot, and evolved chiefly from a study of the cyclones in the Bay of Bengal, makes the condensation of vapour the *primary* cause of disturbance, but allows the greater part of the *subsequent* depression of the barometer to be due to the causes adduced by Ferrel. This latter theory, in fact, only differs from that put forward by the author in the part played by condensation of vapour in giving rise to the initial motion of the air, which Ferrel considers to be considerably less than that exercised by a difference of temperature. Among the conclusions arrived at by the author, and which are generally confirmed by the results of observation, may be noticed the following; but these, it must be remarked, are only strictly true for a regular, symmetrical, and stationary cyclone:—

(*a*) "The wind inclines towards the centre from the direction of the tangent, and the amount of inclination is nearly in proportion to the friction (mainly of the air against the earth's surface)."

(*b*) "The inclination diminishes with the altitude, and therefore at some distance from the earth's surface the gyrations are more circular than near it."

(*c*) "Toward the centre of a cyclone, where the gyratory velocity is greater, the inclination is less, and therefore the path more nearly circular."

(*d*) "The inclination increases with decrease of latitude, attaining its greatest value at the equator, where the air should flow directly towards or from the centre, and there should be no gyrations."

(*e*) "As the motion of the air below in cyclones is *toward* the centre, in the upper regions of the atmosphere it must be nearly circular, but inclined to the tangent a little *from* the centre."[1]

Inertia comes into play where a cyclone is increasing or diminishing in violence, and its effect is to increase the inclination in the former case and diminish it in the latter, but in general the amount is found to be insignificant.

It was stated in the above brief sketch of the theory, that outside the annulus of high pressure surrounding a cyclone the air should move outwards anticyclonically. Ferrel subsequently puts the matter thus: "*Every cyclone is accompanied by a corresponding anticyclone, and the former cannot exist without the latter.*"

The words cyclone and anticyclone are here used quite apart from the question of barometric pressure, and simply mean districts in which the motion of the air is spirally in towards the centre, or out from the centre respectively. Guldberg and Mohn likewise adopt this definition, which is obviously far more scientific than the too common habit of referring to them as regions of low and high pressure.

Mr. Ferrel, however, differs from all previous investigators in thus linking together the cyclone and anticyclone as mutually dependent parts of the same phenomenon. They have hitherto been treated separately, at least in practice, and though the author's conclusion sounds like a simplification, because it makes *one* out of *two*, we scarcely think he has proved the converse to his proposition, viz. that every anticyclone is accompanied by a *corresponding* cyclone, and cannot exist without it.

For example, in the case of such an anticyclone as every winter forms over Central Asia it would be difficult to point out exactly its corresponding cyclone or cyclones, though it is possible, as the author says, that it may be partly due to the overlapping of the anticyclones, which should surround the permanent North Atlantic and Pacific cyclones at this season.

The relation between the barometric gradient and the velocity of the wind in a symmetrical cyclone is given by the following equation:—

$$G = \frac{1076{\cdot}4(2n\cos\psi + \nu)s}{\cos i(1 + {\cdot}004\,t)}\,\frac{P}{P'},$$

where $\nu = \dfrac{s\cos i}{r}$, and G is the gradient in millimetres per sixty geographical miles, s the velocity of the wind in metres per second, n the earth's angular velocity of rotation per second, ψ the co-latitude, i the inclination of the path of the wind to the isobar, $P\,P^1$ the barometric pressure at the given elevation and at the earth's surface respectively, t the temperature in degrees Centigrade, and r the distance from the low centre in kilometres. Where the gradient is given, the velocity of the wind can be conveniently found from the equation

$$S = -\tfrac{1}{2}a \pm \sqrt{\tfrac{1}{4}a^2 + b\,G},$$

where, if the ordinary English units of space and time are used, viz. a mile and an hour, and the gradient is expressed in inches per sixty geographical miles, we have—

$$\begin{cases} a = \dfrac{0{\cdot}52505\, r\cos\psi}{\cos i} \\ b = \dfrac{r(1 + {\cdot}004\,t)}{{\cdot}005262}\,\dfrac{P}{P'} \end{cases}$$

The equation for the gradient in terms of the wind's velocity is substantially the same as that already given by the author in *Silliman's Journal* for 1874, with the exception of the temperature correction, which was there simply referred to in the text.

As the whole question of the author's formula for the gradient has been thoroughly ventilated in his previous

[1] This conclusion only applies to districts within a moderate distance from the centre. At great distances from the centre the radial component predominates, and the air flows nearly directly towards the centre below, and from it above.

works and in the *Zeitschrift für Meteorologie* (Band x.), we need not notice it here except to point out that, assuming the correctness of the formula, the gradient, *cæteris paribus*, should vary (1) directly with the latitude, (2) inversely with the distance from the centre, (3) inversely as the temperature, (4) directly with the amount of inclination.

The foregoing results have all been obtained without considering the term depending on temperature and humidity, and which expresses the effect of the disturbing force necessary to start and maintain the interchanging motions between the interior and exterior portions of the air over a given area. That such a disturbing function is necessary, is evident both from preliminary considerations, and also from the form of the general equations of motion, since they would otherwise be satisfied by the conditions for a state of *rest*. The author enunciates this principle in Part I. Chap. III. where he says: "*There can be no winds then without a disturbance of the static equilibrium by means of a difference of temperature or of aqueous vapour in different parts of the atmosphere.*" And it is important to bear it in mind, if only because we are too often apt to overlook it in the multitude of secondary causes brought to light by a study of atmospheric mechanics. A consideration of this term, in which temperature and humidity are treated jointly, and the former is *assumed* to vary with the distance from the centre, leads to the remarkable conclusion that there are two species of cyclones, one with relatively *warm* centres, the more common case, and the other with relatively *cold* ones.

These cyclones differ specifically from each other chiefly in the way in which the pressure is distributed and the gyrations directed at different altitudes.

In a cyclone with a relatively warm centre the air at the earth's surface moves in a cyclonic spiral round and towards the centre, but as we ascend the gyratory velocity diminishes with the altitude, and the annulus of high pressure approaches the centre, until at a very high elevation the highest pressure of that stratum might even be at the centre, and the air gyrate anticyclonically from it over the whole area at that level. In brief, the cyclonic area becomes smaller, and the anticyclonic larger, as we ascend.

In a cyclone with a cold centre the reverse occurs. At the surface of the earth the initial tendency of the air is to move outwards, and this may be so strong near the surface that there may be only anticyclonic gyrations at this level, with the maximum pressure of the *lowest* stratum at the centre. As we ascend, however, the gyrations round the centre become more and more cyclonic, while the annulus of maximum pressure gradually retreats further and further from it.

There is, besides, according to the theory, an *ascending* motion of the air in the interior part of a warm-centred cyclone, and a descending motion in the exterior part, both generally *small* in comparison with the horizontal motions toward and from the centre. In the case of a cold-centred cyclone these motions are reversed.

Now as a barometer at the earth's surface records simply the integrated effect of what happens in all the strata up to the top of the atmosphere, this might obviously vary in the *same* way for both kinds of cyclones, and so tell us absolutely nothing of such remarkably diverse conditions prevailing at higher altitudes. The behaviour of the air in the warm centred cyclone is what we are accustomed to observe in the case of *most* cyclones, and as they are as often found with relatively *cold* centres as with *warm* ones, the former occurring more frequently in *summer* and the latter in *winter*, it is difficult to understand why the characteristics of the cold-centred cyclone have never yet been found to prevail, at least in moving cyclones. The author indeed offers an explanation of this circumstance, and endeavours also to account for the absence of stationary cold-centred cyclones in regions like Central and Eastern Asia and North America in winter, where the temperature gradient would be remarkably favourable to their production. The fact, however, that in the centres of these regions at this season there is not only no cyclonic tendency of the winds or depression of the barometer, but, on the contrary, a pressure greatly above the normal, seems strangely at variance with what we should expect according to the theory of the cold-centred cyclone, and is hardly satisfactorily explained away as the result of the irregularity and size of the area, combined with the excessive *cold*, which latter is supposed to increase the density and pressure more than the cyclonic tendency diminishes them. The only two cases of cyclones with cold centres which the author seems able to find are the two general wind systems of the northern and southern hemispheres respectively, which "are simply two great cyclonic systems with a cold centre, having the cold poles of the earth for their centres. The motion of the air eastward around and toward the poles in the middle latitudes, giving rise in those latitudes to the normal south-west winds in the northern, and north-west winds in the southern hemisphere, form the cyclones, and the trade-wind region the corresponding anticyclones, with the equatorial calm-belt for the common limit of the two systems. The tropical calm belt and corresponding maxima of barometric pressure near the parallels of 30°, correspond to the similar calm and dividing line between the cyclone and anticyclone in the ordinary and smaller cyclonic systems."

The primary cause of cyclones, according to Ferrel, is a *horizontal temperature gradient*, so that if a portion of the atmosphere is heated or cooled more than the surrounding parts, and the isotherms are approximately circular, we have the initial conditions for a cyclone; but after the disturbances due to such primary causes have set in, secondary causes depending on loss of heat by expansion in ascent, and gain of heat by compression in descent, as well as retardation of cooling where aqueous vapour is being condensed, come into play, which *on the whole* tend to counteract the initial motions.

The condition of the atmosphere *vertically* with respect to temperature and humidity, is thus of great importance in regard to the duration of a cyclone when it has once started.

The author investigates this point at some length, and works out the conditions for cyclone generation in quite a novel manner, from a consideration of both vertical and horizontal temperature gradients. Generally speaking, the condition most favourable to the maintenance of an ordinary cyclone is that the vertical temperature decrement in the interior should be less than in the surrounding regions. This condition is found to be more easily sustained where the air is charged with aqueous vapour, since under these circumstances it cools less rapidly in ascending than when dry. He further points out that where the decrement of temperature in the interior is less than outside, especially when this condition occurs throughout the entire atmosphere, a cyclone may arise without any horizontal temperature gradient (provided only a small instantaneous impulse be given), and that such a state of *unstable equilibrium* more readily occurs when the air is warm and saturated with vapour. While, however, he thus admits the important *rôle* played by vapour in maintaining cyclonic action when once started, he distinctly denies its claim to be considered "*either a primary or principal cause of cyclones.*"

As these islands in all probability seldom, if ever, form the birthplace of a cyclone, but we are rather accustomed to experience them either fully developed or else in the condition of being "filled up," the circumstances which attend their generation do not practically very much concern us. Still it must not be overlooked that conditions which would tend to create and maintain a cyclone in our midst, must of necessity tend to augment the

violence of a storm arriving on our coasts, so that if meteorology, or that branch of it termed weather forecasting, is ever to become an exact science, we must endeavour to find out, by captive balloons or otherwise, what can never be determined by registration at the earth's surface alone, viz. the condition of the atmosphere *vertically* as regards temperature and humidity.

The author concludes his theoretical investigation into the mechanics of cyclones by a discussion of the causes of their motion over the earth's surface. He first of all shows that every cyclone possesses an inherent force tending to urge it towards the pole of the hemisphere in which it has been formed. This follows immediately from the fact that the deflecting force due to the earth's rotation varies with the cosine of the colatitude, and is therefore greater on the polar than on the equatorial side of a cyclone, a residual poleward component of force being thus brought to bear upon every portion of the cyclone. In addition to this, a cyclone, as soon as it is generated, must partake of the general motions of the atmosphere, which the author more especially deals with in his "Meteorological Researches," Part I., to which we have already made allusion; and since the general motions of the atmosphere are there considered to form two great cyclonic systems round the poles, all ordinary cyclones are simply cyclones within a cyclone, so that their general motion of translation is partly the result of the actual motion of the air in these large and perpetual, though perpetually changing, cyclones, and partly that of their inherent tendency to *press polewards*.

For example, as the author says, "in the trade-wind latitudes the wind at the earth's surface is westward . . . and hence the cyclones in these latitudes are carried westward, . . . and having a tendency towards the Pole, the resultant of the two is a westward motion, inclined a little towards the poles, or in the northern hemisphere a motion about west-north-west. After having arrived at the parallel of 30° or 35° in the tropical calm-belt, where there is no westward motion, the progressive motion is a Polar one mostly, but after progressing still nearer the Pole, into the middle and higher latitudes, the general eastward motion of the atmosphere here, which is great in the upper regions, carries now the cyclone toward the east, and the direction of the progressive motion, which is usually about east-north-east, is the resultant of this eastward motion and the motion round the Pole. All well-developed cyclones, therefore, having their origin near the equator, have mostly a progressive motion represented by a curve somewhat in the form of a parabola, having its vertex in the tropical calm belt at the parallel of 30° or 35°."

It is moreover shown that the general motions of the atmosphere must not only cause the cyclone to travel more or less with them, but also affect the inclination of the wind to the isobars, decreasing it in the front, and increasing it in the rear part.

With regard to further modifying causes, the author favours the views of Clement Ley regarding the effect of the distribution of aqueous vapour in determining the direction in which a cyclone propagates itself.

He does not indeed attempt to explain how they sometimes wander off on an entirely unlooked-for course, or else remain stationary for some considerable period; otherwise he might claim to have at least attempted a solution of the entire problem on which weather science depends. Clement Ley himself, in his admirable little work,[1] recently published by authority of the Meteorological Council, tells us that the reason why the course of a cyclone cannot be exactly foretold is because "in the first place the causes which determine the course of depressions are not fully known; in the second, place so far as they are known, it is certain that the course of depressions is generally related *to the distribution of pressure over a very large area.*" In fine before we know how the *small* cyclones are going to behave, we must in every case know the *form* of the larger cyclones round whose centres they travel.

E. DOUGLAS ARCHIBALD

(*To be continued.*)

[1] "Aids to the Study and Forecast of the Weather," by W. Clement Ley, M.A., 1880.

THE GIZZARD-CONTENTS OF SOME OCEANIC BIRDS

THE following results of the examination of the gizzards of twenty sea-birds, which were caught by the officers of this ship in the South Atlantic and Southern Indian Oceans during the last quarter of 1881, may be of interest to some of the readers of NATURE.

With one exception all the birds belonged to the Petrel family—the Procellaridæ—and fifteen of them were of the well-known species—the Cape-pigeon or Cape-petrel (*Daption capensis*). The most frequent of the gizzard-contents of these twenty birds were the mandibles of a cephalopod, which were found in eighteen instances; the otoliths of some small osseous fishes occurred in five instances; and some curious stony masses, varying in weight from half a grain to five grains, were obtained also in five instances. The other substances, which were observed less frequently, were the vertebræ of a fish, feathers, Velellæ, the horny rings probably of some crustacean, and a small hard seed.

With reference to the seed just mentioned, I should observe that it was taken from the gizzard of a Cape-pigeon, about 550 miles to the east of Tristan d'Acunha, in the South Atlantic. The wide range of this species of petrel is well known; we ourselves first observed this bird rather to the southward of the island of Trinidad, which lies about 600 miles off the coast of Brazil; and thence we traced it as far as the island of Amsterdam, in the southern portion of the Indian Ocean. From our own observation, therefore, it is quite possible that a seed might be transported from Trinidad to Amsterdam, notwithstanding that these islands are from five to six thousand miles apart; and Mr. Mosely's surmise (*vide* a footnote in Mr. Wallace's "Island Life," p. 250) that various species of Procellaria and Puffinus may have played a great part in the distribution of plants, and may to some degree explain the similarity in the mountain floras of widely distant islands, would appear to receive some support from the single instance of this seed. With regard to the kind of plants to which the seed belongs, Mr. Moore, director of the Botanic Gardens, Sydney, kindly informed me that it possessed no character sufficiently distinctive to enable him to decide as to its probable source.

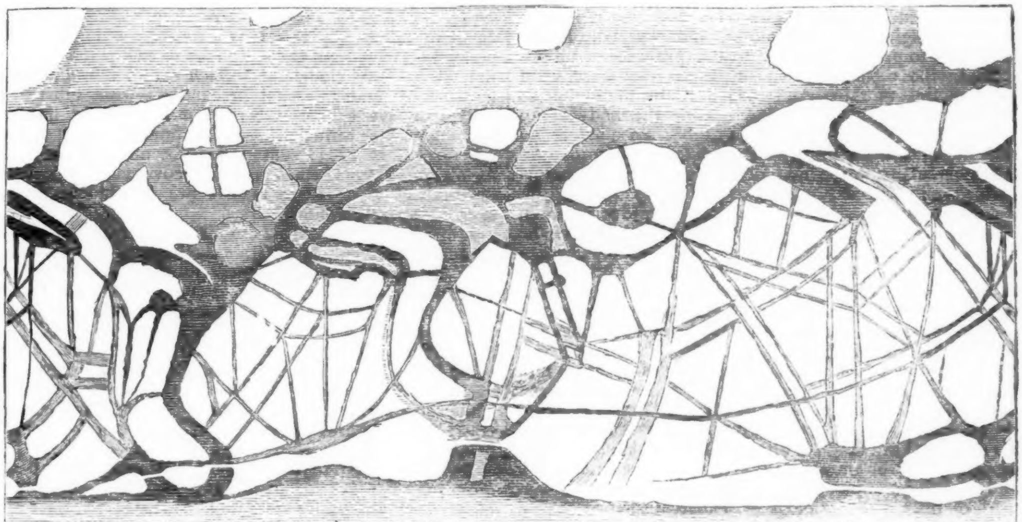
The stony masses found in the gizzards of five of the birds, all of which were caught in the South Atlantic, were of two kinds: one of the masses was of a dark colour and homogeneous texture, and rather porous; when heated it gave off black fumes with a smell of burnt organic matter, and was fusible with soda into a black glass; the other masses had the appearance of greasy quartz, scratching glass with ease; but when heated in a closed tube they blackened and evolved black fumes with a powerful odour of burnt animal matter; after the incineration they became white, and with the blowpipe were fused into a white glass after the addition of soda; no effervescence was exhibited on the application of an acid. The behaviour of these masses under heat is very similar to that described by Mr. Darwin in his "Geological Observations," in the case of a stony incrustation on St. Paul's Rocks, deposited, as he considered, from water draining through birds' dung.

H. B. GUPPY

H.M.S. *Lark*, Auckland, February 28

RECENT DISCOVERIES IN THE PLANET MARS

An intended article, of which an announcement appeared in NATURE a few weeks back, on the topography of Mars as delineated for the second time by Prof. Schiaparelli at Milan during the opposition of 1879-80, has been anticipated and in part superseded by information which has been received relative to the more recent discoveries made by him in the beginning of the present year. Pending the preparation of a fuller and more detailed memoir, he has published a preliminary notice, read before the Academia dei Lincei on March 5, and accompanied by a photographed drawing of the planet's surface. The results are of a very remarkable and unexpected character; and as through the courtesy of this distinguished observer, the notice and photograph have been placed in my hands, I am induced to reproduce the latter, which, though not pretending to minute accuracy (the original, in fact, is only a provisional sketch), will give a sufficient idea of the marvellous duplication of the so-called "canals," which, between January 19 and February 24, in about twenty instances, unfolded itself progressively under the observer's eye. The discussion which took place at the late meeting of the Astronomical Society, so far as my information extends, substantiated strongly by independent evidence the existence of these long, narrow streaks, some of them even in positions where they have not been delineated by Schiaparelli; but their duplication by similar and parallel lines does not seem to have been elsewhere noticed. Some difference of opinion may possibly be expected concerning these strange appearances; and the consequent enfeebling (to say the least of it) of the long-admitted terrestrial analogy may be, to some minds, unacceptable; but the established reputation of the observer demands at any rate a respectful attention to his statements. It may be preferable to suspend a more detailed account till we receive a full elucidation of the subject in the memoir, of which we possess only a preliminary notice; for the present it may suffice to mention that he found the atmosphere of Mars apparently clearer than in 1877, and was thus enabled to recover the markings then detected more satisfactorily



even than in 1879-80, and to confirm the general accuracy of his two earlier charts; while the concise, but very clear intimations that he has given, as to the variable brightness of some great regions, the progressive enlargement on one side since 1879 of the "Kaiser Sea" (his Syrtis Magna), the brightening of certain supposed continents or islands towards the limbs, the confirmed existence of oblique white streaks, the unfolding of minute labyrinthine detail, and the continuous development already mentioned, day after day, of the collateral lines which double the so-called "canals," and extend with them ordinarily along great circles of the sphere—all these and similar announcements make us anxiously desire a more extended and detailed communication. For some of these most remarkable appearances parallels may be to a certain extent produced from the results of earlier observers; but, so far as at present appears, the duplication stands alone. The discoverer is disposed to infer a connection between these progressive developments and the seasons of the planet, and on that account hopes that, owing to the position of the axis at the ensuing opposition at the opening of 1884, notwithstanding the diminished diameter (only 12″·9), confirmation of his announcements may be obtained from other observers. We sincerely trust that a report which has reached us may be verified as to the erection of a much larger telescope in the Royal Observatory at Milan, and that the extraordinary talent and diligence of the director may be richly rewarded, not only by the confirmation but the extension of results which must so materially influence our conclusions as to the physical condition of this peculiarly interesting planet.

T. W. WEBB

THE CAUSE OF TUBERCULOSIS

The first step in the rational treatment of every malady is obviously the recognition of its cause, and it is this recognition which enables medical men to do battle against disease. It is a truism to say that as regards infectious maladies the knowledge of their cause is an essential step in preventing their spread and arresting their ravages. The malady known as tuberculosis, and generally characterised by constitutional disturbance associated with the production of minute nodular new-growths in the various organs, especially the lungs, spleen, lymphatic glands, serous membranes, the membranes of the brain, liver, &c.—[at first greyish and transparent, but

afterwards becoming opac and degenerating into a yellowish-looking *débris*, and hereby implicating and destroying the organs in which they are located]—has been shown to be an infectious malady communicable from one human being to another, from man to animal, and from animal to animal.

The successful experiments of inoculating with, feeding on, and causing to inhale human tubercular matter, carried out on the lower animals, such as guinea-pigs, rabbits, dogs, pigs, &c., by Villemin, Dr. Wilson Fox, Mr. John Simon, and Dr. Burdon Sanderson, but especially by Cohnheim and Salomonsen, Tappeiner, and Baumgarten are conclusive in these respects. Similarly it has been shown that the tuberculosis of cattle or Perlsucht is communicable not only within the species but also to other animals. Whether Perlsucht is also communicable to man, especially through meat and milk, as is maintained by some observers (Semmer, Baumgarten, and others), is as yet an open question, and, as must be obvious to every one, one to which fearful importance is attached, considering how great the distribution of this disease is in the bovine species. What the cause of the malady is, has until now been undetermined, although it has been at various times surmised that, like other infectious diseases, it is of a parasitic origin. Thus Schüller, and especially Klebs, have tried to prove that owing to the presence of micrococci in the tubercular deposits, these micrococci were the materies morbi. And indeed Klebs maintains to have succeeded in cultivating outside the body of an animal, *i.e.* artificially the "monas tuberculosum," as he calls the said micrococcus, and to have again produced the tubercular disease by inoculating animals with this purified micrococcus. Klebs' observations and conclusions have not been accepted as reliable, and it has been reserved for Dr. Koch to discover the real cause of the disease, in identifying it with a specific bacillus. In a weighty paper, "The Etiology of Tuberculosis," published in the Berlin *Klin. Wochenschr*, 1882, No. 15, Dr. Koch sets forth the whole course of his investigation, the methods and experiments, all his observations and definite conclusions on this question, and anyone who peruses carefully this paper will come to the conclusion that Koch's results are to be accepted with unconditional faith, and I have no manner of doubt will be considered by all pathologists as of the very highest importance. To those who are familiar with Dr. Koch's previous work, especially that on the etiology of splenic fever or Anthrax, and his observations on pathogenic Bacteria, this last work of his, on the Etiology of Tuberculosis, will be an additional and brilliant testimony to his ingenious and successful method of research.

The first step in the inquiry was to ascertain whether any definite form of microphyte is constantly present in the tubercular deposits. This question could not be solved by the ordinary methods of research, but with new methods; it was decided in the affirmative. For the demonstration of the presence of the specific bacillus—which Koch calls the tubercle-bacillus—the following method proved successful: Tubercular deposit fresh, or after hardening with reagents, is stained for twenty to twenty-four hours—at a temperature of 40 Centigrades, only half to one hour is required—with a half per cent. solution of methylene blue, to which a small quantity of a 10 per cent. solution of caustic potash is added. After this, the tubercular material is stained for a minute or two in a concentrated watery solution of vesuvin, and then washed in distilled water. When examined under the microscope, all elements of the tubercular deposit, such as cells, nuclei, fibres, and granules, appear of a brownish colour, while the tubercle-bacilli alone stand out very conspicuously in a beautiful blue tint.

By this method Koch ascertained the constant presence of the specific bacillus in the tubercular eruption in man and animals, including the Perlsucht of cattle, both in spontaneous tuberculosis, as well as artificially produced, *i.e.* by inoculation. These bacilli differ from all other micro-organisms by characteristic properties.

The next step in the inquiry was one of essential importance in determining the nature of the bacillus as the materies morbi, viz. to isolate by successive cultivations outside the animal body, the tubercle-bacilli, and having thus completely freed them of all parts of tissue of the tubercular deposit to introduce them into the system of suitable animals. If these animals became afflicted with typical tuberculosis, and if at the same time similar animals kept under precisely the same conditions, but not infected with the bacilli, remained perfectly normal, it will be admitted that the exact proof has been given that the bacilli constitute the cause of the tubercular malady.

All these conditions have been fulfilled by Dr. Koch in an eminent degree.

The tubercle-bacilli were successfully cultivated outside the body of an animal. Pure serum of blood of sheep or cattle is sterilised by keeping it exposed in test-tubes plugged with cotton wool, for six days daily for one hour, to a temperature of 58 centigrades. After this the serum is heated for several hours up to a temperature of 65 centigrades; by this it is transformed into a solid perfectly transparent mass, well adapted for the cultivation of the tubercle-bacilli. Such serum inoculated on its surface under special precautions with tubercular matter of any source—tuberculosis of man or animal, spontaneous or artificially produced, and kept at a temperature of 37 or 38 centigrades (*i.e.* about blood-heat) for over a week, becomes gradually covered with peculiar dry scaly masses; these masses are the colonies of the specific tubercle-bacillus.

A minute particle of this crop is used for establishing a second similar cultivation, this again for a third, and so on. Tubercle-bacilli obtained in this manner, after several successive generations, prove as effective in inoculating animals with typical tuberculosis as fresh tubercular matter.

All animals susceptible to the malady that Koch inoculated with these artificially cultivated bacilli, became invariably affected with the disease; not one escaped; while other similar animals kept under precisely the same conditions, except that they did not receive any tubercle-bacilli, remained perfectly healthy.

It is important to notice that the tubercle-bacilli require for their growth and multiplication a temperature of at least 30 centigrades, and, consequently, they are limited to the animal body, unlike the bacillus that produces splenic fever or anthrax, which is capable of multiplication at ordinary temperatures, as low as 20 centigrades, and even less.

The expectorated matter of tubercular patients is generally charged with tubercle-bacilli, and these often contain spores. And it is probably through the presence of such spores that that matter retains for a long time its infective properties. Koch made experiments on guinea-pigs with such sputa after having been kept dry for fourteen days, for four weeks, and for eight weeks, and he found that in all instances the sputa had retained their full virulence. It is therefore just to assume that such sputa, even when dried on linen, clothes, or even distributed with the dust of the room, may be a source of infection.

The practical importance of the discoveries of Koch must be patent to everybody. In the recognition of the tubercle-bacilli as the *fons et origo* of this terrible pest of the human race—according to statistics quoted by Koch one-seventh of all deaths being caused by tuberculosis—in the recognition of the presence of these bacilli in the sputa of tubercular patients, and in the tubercular deposits of cattle afflicted with Perlsucht, we have at once become supplied with the knowledge of the most common manner of how tuberculosis may and probably is spread, as well

as with the weapon of combating the most fertile sources of infection. In preventing the distribution—either by proper disinfection, or by destruction—of the expectorations of tubercular persons, and further, in superintending and restricting the use of tubercular animals of the bovine species, we possess the means of preventing the spread of this deadly and terrible plague, and hereby saving a vast amount of human life. These discoveries of Dr. Koch were made entirely through experiments on living animals.

E. KLEIN

DEEP-SEA EXPLORATION

IN NATURE, vol. xviii. p. 348, we described Negretti and Zambra's Patent Deep-Sea Standard Thermometer. Some uncertainty as to the accuracy of its indications in deep sea service led to a re-arrangement of the instrument, which now greatly increases its reliability. The improvement is chiefly due to suggestions furnished by Commander Magraghi (see NATURE, vol. xxiv. p. 505)

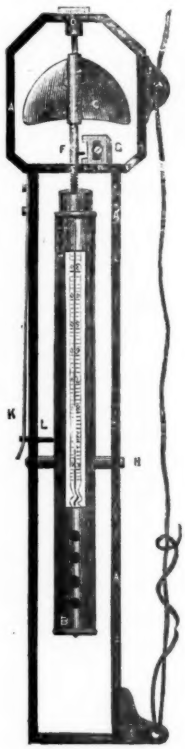


FIG. 1.

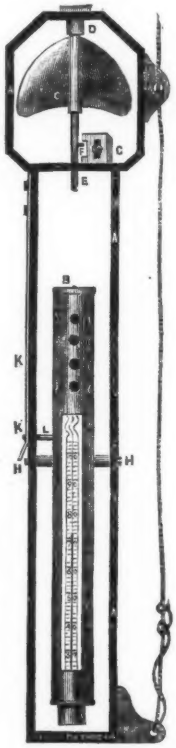


FIG. 2.

FIG. 3.

(of the Royal Italian Navy) to Negretti and Zambra. Several of these improved thermometers may now be fastened on one line, and serial temperatures at any required depth obtained with certainty.

The woodcuts exhibit the apparatus, Fig. 1, as prepared for lowering down into the Sea, and Fig. 2 after the hauling up has commenced—the thermometer having reversed and registered the temperature at the moment of turning over. Fig. 3 shows the peculiar construction of Negretti and Zambra's inverted thermometer used in their improved deep-sea apparatus. The apparatus will be understood by reference to figures (Nos. 1 and 2). A is a metal frame, in which B, the thermometer, is pivoted upon an axis, H, but not balanced upon it. C is a screw-fan attached to a spindle, one end of which works in a socket, D, and at the other end is a screw, E, about half an inch long, and just above it is a small pin, F. On the spindle G, is a sliding stop-piece, against which the pin, F, impinges when the thermometer is adjusted for use. The screw, E, works into the end of the case, B, the length of play to which it is adjusted. The number of turns of the screw entering the case is regulated by means of the pin, F, and stop-piece, G. The thermometer and its case is held in position by the screw, E, and descends into the sea in this position—as Fig. 1; the fan, C, not acting during the descent, because it is checked by the stop, F. When the ascent commences, the fan revolves, raises the screw, E, and releases the thermometer, which then turns over and registers the temperature at that spot. When the hauling-up has caused the thermometer to turn over, a spring at K forces the pin, L, into a slot in the case B, and clamps it (as seen in Fig. 2) until it is received on board, so that no change of position can occur during the ascent from any cause. The case, B, is cut open to expose the scale of the thermometer, and also perforated to allow free passage of the water.

SOME PRIMITIVE IDEAS ON METEOROLOGY

IN an article published in NATURE (vol. xxv. p. 82) on the opinions of the Chinese Emperor Khang-hi on certain natural phenomena, it will be remembered that the *yang* and *yin*, or the male and female principles of Chinese philosophy, played a conspicuous part. Japan, it is well known, adopted at a very early period in its history the law, polity, science, philosophy, and writing of the Chinese, and with them the *yang* and *yin*; and it may not be uninteresting to our readers to see how the doctrine of these dual forces, mutually repellent as well as attractive, has been employed to explain the facts of meteorology. A recent issue of the *Japan Gazette* newspaper of Yokohama contains the translation of a work written in 1821 by a certain Arai Yoshinari, called the "*Ten-chi-jii*; or, Ideas about Heaven and Earth." The heavens, the writer says, are very high, the earth is very thick; we cannot ascend to the one or go down into the other; consequently man was unable for many generations to comprehend the phenomena of either; but now the opinions of all philosophers on this subject are based on the action and reaction of the male and female, the active and passive principles of nature upon each other. The rain is a changed form of the male, and the vapour under the earth of the female principle. When the male principle sinks into the earth it pursues the female. The earth is the mother of all things and the heaven is the air or wind where the sun, the moon, and the stars hang shining. There are two kinds of air—the heaven-air and the earth-air. The motion of the heavens is contrary to that of running water. The heavens move from east to west, while water runs from west to east. In some districts, indeed, water in the earth runs towards the north, but meets the earth-air which obstructs its flow, causes much agitation, and finally its complete evaporation from the surface of the earth. The vapour thus formed ascends and becomes clouds, which are again turned into rain by the action of the wind. The water has periods of increase and decrease according to the male and female seasons; thus in summer, which is the male season, water increases, while in winter, or the female season, it diminishes. Again, the earth-air is changed into rain when it moves from east to west; and therefore, previous to rain, we see a white vapour in the morning ascending in the east. "This is a clear proof of the earth's growing hot." For the same reason mountains become somewhat darker just before rain.

Thunder is produced by the mingling of the male and female principles. Sounds are often heard in the earth in the neighbourhood of volcanoes. This is due to the

irritation of the earth-air, which sometimes sends out flames. It is said that a kind of beast accompanies the thunder, and it moves about in the air. This is nothing strange, because at a certain island called Ampon, which is about 3900 *ri* (1 *ri* = $2\frac{1}{2}$ miles) from Japan, there is a bird called the *Kasubara*, which is covered with fur instead of feathers, and which eats fire. Other birds live on wind. As this world is unlimitedly great and extensive there may have lived strange beasts and birds, like the thunder beast which the Japanese talk about. The volume of sound given out by thunder depends on the number of water-clouds in the air. When the latter is small, the sound of the thunder is not loud and appears far off. On the other hand when the clouds are piled up in the heavens, the sound is loud and is simultaneous with the lightning. The sound is caused by the passage of fire through the water. The ancients regarded thunder as the report of the battle between fire and water—the male and female elements. If this were the case there is no reason for the interval between the flash and the sound. Earthquakes are subterranean thunder; the noise is caused by the rush of water which has long been kept confined by the earth-air. Snow is the vapour which rises from the earth; when it ascends high enough it becomes frozen and falls as snow. Fog is also this vapour. Haze is the vapour mixed with smoke from some volcano. The writer concludes by expressing his intention of making the actions of nature, such as rain, wind, &c.—difficult as they are to explain—quite clear on a future occasion.

These ideas may be taken as representing those of most educated Japanese of half a century ago, with the exception perhaps of a few who had been taught by the Dutch. What the Japanese peasant thought, and still thinks of thunder, earthquakes, storms, and other striking natural phenomena will be found in a deeply interesting chapter of Mr. Griffis's "Mikado's Empire." One of the principal Japanese artists, Hokusai, some of whose works have recently been given to the English public, did not think it beneath his genius to endeavour to picture the extraordinary creatures that form the zoological mythology of Japan. There the astonished student of Japanese pictorial art can behold Futen, the wind demon, Raiden, the creator of thunder, the fish whose movements cause earthquakes, the *kappa*, or demon of the deep, and dragons of sufficient variety of form to satisfy the weirdest imagination.

NOTES

RARELY has so distinguished and representative an assembly been seen in Westminster Abbey as that which met to pay the last honours to Mr. Darwin, on Wednesday last week. The Abbey indeed was crowded. The character of the long line of distinguished men who followed the honoured remains to the grave, may be seen from the list of pall-bearers:—The Duke of Devonshire, the Duke of Argyll, the Earl of Derby, Mr. J. Russell Lowell, the American Minister, Dr. W. Spottiswoode, P.R.S., Sir Joseph Hooker, Mr. A. R. Wallace, Prof. Huxley, Sir John Lubbock, and the Rev. Canon Farrar. Mr. Darwin has been buried close beside the grave of Sir John Herschel, and within two paces of that of Sir Isaac Newton. At the Royal Academy dinner on Saturday, Mr. Spottiswoode, in replying for science, could not but refer to the loss "of our greatest philosopher and noblest spirit." "I know not," he said, "whether, in the presence of statesmen and leaders of thought, of commanders both by sea and land, of artists, of preachers, of poets and men of letters of every kind, it is fitting that I should speak of greatness; but if patience and perseverance in good work, if a firm determination to turn neither to the right hand nor to the left, either for glory or for gain, if a continual overcoming of evil with good in any way constitute elements of greatness, then the man of whom I speak—Charles Darwin—was truly great. He lived, indeed, to a good age; he lived to complete the great work of his life; he lived to witness a revolution in public opinion on matters with which he was concerned such as few had seen before—a revolution from opposition to concurrence, a revolution from antipathy to sympathy, or whatever else may better express a complete change of front. And so having at the beginning been somewhat rudely pushed aside as an intruder and disturber of accepted opinions, he was in the end not only borne on the shoulders of his comrades to his last resting-place, but was welcomed at the threshold by the custodians of an ancient fabric and of an ancient faith as a fitting companion of Newton and of Herschel, and of the other great men who from time to time have been gathered there."

M. JAMIN, president of the Academy of Sciences, having summoned M. Quatrefages to deliver an *éloge* on the late Mr. Charles Darwin on Monday last, the eminent zoologist read a long and eloquent oration, which was received with unanimous plaudits, and will be printed in the next *Comptes Rendus*.

WE take the following from the *Times*:—The Council of the Royal Society have selected the following fifteen from the fifty-two candidates for the Fellowship who have presented themselves during the present session. The election, which rests with the Fellows of the Society, will take place on Thursday, June 8, at 4 p.m. The names are—Prof. V. Ball, Dr. G. S. Brady, Dr. G. Buchanan, C. Baron Clarke, Francis Darwin, Prof. W. Dittmar, Dr. W. H. Gaskell, Mr. R. T. Glazebrook, Mr. F. Ducane Godman, Mr. J. Hutchinson, Prof. A. Liversidge, Prof. I. Malet, Mr. W. D. Niven, Mr. R. H. Inglis Palgrave, and Mr. W. Weldon.

THE fifty-second Annual Meeting of the British Association for the Advancement of Science will commence in Southampton on Wednesday, Aug. 23. The President-Elect is C. W. Siemens, D.C.L., F.R.S. Vice-Presidents-Elect: The Right Hon. the Lord Mount-Temple, Capt. Sir F. J. Evans, K.C.B., F.R.S., Hydrographer to the Admiralty, F. A. Abel, C.B., F.R.S., Prof. de Chaumont, M.D., F.R.S., Col. A. C. Cooke, R.E., C.B., Director-General of the Ordnance Survey, Wyndham S. Portal, Prof. Prestwich, M.A., F.R.S., Philip Lutley Sclater, F.R.S. General Treasurer: Prof. A. W. Williamson, F.R.S., University College, London, W.C. General Secretaries: Capt. Douglas Galton, C.B., D.C.L., F.R.S., Francis Maitland Balfour, F.R.S. Secretary, Prof. T. G. Bonney, F.R.S. Local Secretaries: C. W. A. Jellicoe, John E. Le Feuvre, Morris Miles. Local Treasurer, J. Blount Thomas. The Sections are the following: A—Mathematical and Physical Science—President, Right Hon. Prof. Lord Rayleigh, F.R.S. Vice-Presidents: G. H. Darwin, F.R.S., Prof. G. C. Foster, F.R.S. Secretaries: W. M. Hicks, M.A., Prof. O. J. Lodge, D.Sc., D. McAlister, M.A., B.Sc. (Recorder), Rev. G. Richardson. B—Chemical Science—President, Prof. G. D. Liveing, F.R.S. Vice-Presidents: A. G. Vernon Harcourt, F.R.S., Prof. H. E. Roscoe, F.R.S. Secretaries: P. Phillips Bedson, D.Sc. (Recorder) H. B. Dixon, F.C.S., J. L. Notter. C—Geology—President, R. Etheridge, F.R.S. Vice-Presidents: Prof. T. Rupert Jones, F.R.S., Prof. J. Prestwich, F.R.S. Secretaries: T. W. Shore, F.G.S., W. Topley, F.G.S. (Recorder), E. Westlake, F.G.S., W. Whitaker, F.G.S. D—Biology—President, Prof. A. Gamgee, M.D., F.R.S. Vice-Presidents: Prof. W. Boyd Dawkins, F.R.S., G. E. Dobson, F.L.S., Prof. M. A. Lawson, F.L.S., Prof. J. D. Macdonald, F.R.S. Department of Anatomy and Physiology:—Prof. A. Gamgee, M.D., F.R.S. (President), will preside. Secretaries: W. Heape, A. Sedgwick, B.A. (Recorder). Department of Zoology and Botany:—Prof. M. A. Lawson, F.L.S. (Vice-President), will preside. Secretaries: W. A. Forbes, F.Z.S. (Re-

Recorder), J. B. Nias. Department of Anthropology:—Prof. W. Boyd Dawkins, M.A., F.R.S., F.S.A., F.G.S. (Vice-President), will preside. Secretaries: G. W. Bloxam, M.A., F.L.S.) (Recorder), T. W. Shore, jun., B.Sc. E—Geography:—President: Sir R. Temple, Bart, G.C.S.I. Vice-Presidents: H. W. Bates, F.R.S., Lieut.-Col. H. H. Godwin-Austen, F.R.S. Secretaries: E. G. Ravenstein, F.R.G.S., E. C. Rye, F.Z.S. (Recorder). B—Economic Science and Statistics:—President: Right Hon. G. Sclater-Booth, M.P., F.R.S. Vice-Presidents: W. E. Darwin, F.G.S., R. H. Inglis Palgrave, F.S.S. Secretaries: G. S. Baden-Powell, F.S.S., Prof. H. S. Foxwell, F.S.S., A. Milnes, M.A., F.S.S., Constantine Molloy (Recorder). G—Mechanical Science:—President: John Fowler, C.E., F.G.S. Vice-Presidents: A. Giles, C.E., W. H. Preece, C.E., F.R.S. Secretaries: A. T. Atchison, M.A., F. Churton, H. T. Wood, B.A. (Recorder). The First General Meeting will be held on Wednesday, August 23, at 8 p.m. precisely, when Sir John Lubbock, Bart., M.P., F.R.S., will resign the Chair, and C. W. Siemens, D.C.L., F.R.S., President elect, will assume the Presidency, and deliver an address. On Thursday evening, August 24, at 8 p.m., a *soirée*; on Friday evening, August 25, at 8.30 p.m., a Discourse on Pelagic Life, by Prof. H. N. Moseley, F.R.S.; on Tuesday evening, August 29, at 8 p.m., a *soirée*; on Wednesday, August 30, the concluding General Meeting will be held at 2.30 p.m.

It may be useful for some of our readers to be informed that the following arrangements have been made by the American Association for the Advancement of Science for reduced fares from Europe to Montreal, for those attending the meeting on August 23 next:—The Allan Line will grant ten tickets at $100 each from Liverpool to Quebec and return; the Dominion Line will grant twenty-five tickets at $80 each from Liverpool to Quebec and return; the Beaver Line will grant tickets from Liverpool to Quebec and return at $80 each.

The eleventh meeting of the French Association for the Advancement of Science will take place at Rochelle, commencing August 24. The General Secretary is Prof. Gariel, 4, rue Antoine Dubois, Paris.

The honorary degree of LL.D. has been conferred on Mr. J. R. Hind, F.R.S., by the University of Glasgow.

The death is announced, at the age of forty-eight, of the well-known physicist Prof. Zöllner, of Leipsic.

Lady Thomson, widow of Sir Wyville Thomson, is to receive a grant of 400*l*. from the Royal Bounty Fund.

The French Eclipse Expedition has arrived at Alexandria.

On April 27 the French Academy received M. Pasteur, who has been nominated to fill the chair vacated by the recent death of M. Littré. The ceremony attracted an immense concourse of people, including the *élite* among French *savans* and politicians. M. Pasteur delivered an eloquent address against the opinions of his predecessor, who was partly defended by M. Renan. The two speeches are among the most interesting and elaborate that have been delivered under such circumstances.

We have received, as specimens of the seismological literature of Japan, reprints of certain translations which have appeared in the *Japan Gazette* newspaper. The first is the narrative of an earthquake shock at Osaka, accompanied by a high wave, in 1707; the second, a similar narrative of a great earthquake in the province of Echigo in 1829; and the third an earthquake chronology. The editor, Prof. Milne, speaks of the first as little more than a series of anecdotes of various events which took place at the time of the disaster; and although the seismologist may not be able to glean many facts of value, the paper will at least give him a specimen of the kind of literature through which he will have to wade in searching for facts of scientific importance. He adds that he is acquainted with sixty-five Japanese works on the subject, and that in Japan there is a literature on earthquakes comparable with that of any other country, and although much of it may be of interest only to the general reader, much of it has a value scientifically. The second monograph is interesting, on account of the many references it contains to popular beliefs respecting the connection between earthquakes and other natural phenomena. Thus, an unusual warmness in the weather, a change in the colour of the moon, mirage, falling stars, &c., are all referred to as being connected with the approach of an earthquake. The third paper is a translation of an earthquake Calendar, commencing at 295 B.C. and ending with the widespread and destructive earthquakes of 1854. This work shows that, notwithstanding the frequency of these phenomena in Japan, the native chroniclers have always carefully recorded them. Probably nowhere else in the literature of the world can we find so long and complete a record of the recurrence of various natural phenomena—for eclipses, great waves, volcanic eruptions, &c., are also noted—than in this work.

A series of three excursions has been arranged by the Geologists' Association, to afford members an opportunity of becoming acquainted with the physiography and geological character of the Weald. The first excursion, on May 6, will be to Redhill and Crawley; the second, May 30, to Tilgate Forest, Cuckfield, and Hayward's Heath; and the third, May 29 and 30, to the Isle of Purbeck.

The annual general meeting of the Iron and Steel Institute will take place on May 10, 11, and 12. The papers to be read are:—On certain physical properties of iron and steel, by Mr. Edward Richards, Hematite Steel Works, Barrow-in-Furness; On the use of brown coal in the blast furnace, by Prof. Ritter Peter von Tunner, Leoben, Austria; On the Bilbao iron ore district, by Mr. William Gill, M.I.C.E., Luchana, Bilbao, Spain; On a new method of getting coal, by Mr. Paget Mosley, London; On the compression of fluid steel, by Mr. William Annable, Govan, Glasgow; On the chemical composition and testing of steel rails, by Mr. G. J. Snelus, F.C.S., A.R.S.M., Workington; On the consumption and economy of fuel in iron and steel manufacture, by Mr. J. S. Jeans, London; On the tin plate manufacture, by Mr. Ernest Trubshaw and Mr. E. S. Morris; On the relations of carbon and iron, by Mr. Geo. E. Woodcock, Atlas Works, Sheffield; On a new centre crane for Bessemer plant, by Mr. Thomas Wrightson, M.I.C.E., Stockton-on-Tees.

On April 30 M. Carlier, one of the most active members of the Académie d'Aërostation Météorologique, made an ascent at the La Villette gasworks, Paris, in order to try if it is possible to steer a balloon by using in the car a large oar composed of a plane fixed perpendicularly to a solid handle worked with two hands. The dimensions of the plane are one metre by two, and the handle is about three metres long. The weight of the sails is counterpoised when worked, and the weight of the whole system is about 10 kilograms. It is the second time that M. Carleer has ascended with this apparatus. Although the air was in a state of great agitation the motions of the balloon were easily seen from the ground. M. Carlier intends to make a series of ascents in order to learn how to make the best of this system, which is to be used only for partial direction, as in the case of Thames barges, which, although they must follow the run of the tides, can be directed to some extent by means of the oars.

The May number of the *Proceedings* of the Royal Geographical Society contains a paper of much interest by Mr. L. K. Rankin, B.A., on "The Elephant Experiment in Africa." Mr. Rankin

accompanied Capt. Carter on his journey with the three Indian elephants in 1879, meant for the use of one of the Belgian expeditions. In his paper Mr. Rankin gives full details of the conduct of the elephants up to Mpwapwa, where their troubles began. Although they were severely attacked by the Tsetse, no permanent evil effect seems to have followed. At Mpwapwa, indeed, a report was sent to the King of the Belgians, in which it was stated that the elephant experiment was a complete success, on account of their immunity against Tsetse, their ability to live on the uncultivated food of the country, and to march over all kinds of ground. A few days after the report, however, the largest elephant suddenly died. Mr. Rankin attributed its death to insufficient food and over-work. In India it had been stall-fed; in Africa it never seems to have had enough to eat—the back-bones of all these stood six or seven inches from their flanks at Mpwapu. It is clear also that their loads were far beyond what they had been accustomed to. As is known, the other elephants subsequently died. This experiment cannot be considered a fair one, though the lessons it taught will be of service in any future attempt to utilise the animal as a beast of burden in Africa.

A HYDROMOTOR recently invented by Herr Fleischer of Kiel, for propulsion and steering of vessels, acts (we learn from Wiedemann's *Beiblätter*, 3) by pressure of steam on water, in a cylinder, forcing out the water as a jet below. A float on the water in the cylinder works, in a simple way, the opening and closure of the valves for admission and escape of the steam, and the vacuum produced by condensation of steam in a condenser opens valves for readmission of water. The hot water layer, which forms on the liquid surface, and the wooden lining of the cylinder, reduce the condensation during expulsion of the water to a minimum. A comparison of the working of the author's vessel with that of the *Water-witch* and *Rival* (also propelled by hydraulic reaction) showed that while the kinetic energy of the expelled water was in the *Water-witch* 31·5 per cent. of the indicated quantity of steam, and in the *Rival* 26·3 per cent. in the (so-called) "hydromotor" it was 89 per cent. Herr Flischer, in a recent *brochure*, investigates the physics of his motor.

IF those members of the Quekett Microscopical Club who intend to be present on the occasion of the opening of Epping Forest by Her Majesty on Saturday next, the 6th inst., will communicate with the Hon. Sec. of the Quekett Microscopical Club, 7, The Hill, Putney, S.W., he will do his best to find places for their accommodation.

WITH reference to our notice of "Through Siberia" (vol. xxv. p. 582), the Rev. H. Lansdell writes that in the List of Illustrations at the commencement of vol. i. he acknowledges the sources whence they are taken; and with reference to the photograph of a "Buriat girl" he states that he bought the photograph, in the Buriat country, of the man who took it, that the girl was known even by name to his local friends, and that he has every reason to believe she was a pure Buriat and not a metis.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythræus* ♂) from India, presented by Mrs. Lamprey; a Chinese Tiger (*Felis tigris* ♂) from China, presented by Mr. G. Brown; two Bauer's Parrakeets (*Platycercus zonarius*) from Australia, presented respectively by Mr. J. Charlton Parr, F.Z.S., and Miss Eva Maitland; a Mississippi Alligator (*Alligator mississippiensis*) from Florida, U.S.A., presented by Master Bennett; a Slow-worm (*Anguis fragilis*), British, presented by Mr. Poyer Poyer; two Axolotls (*Siredon mexicanus*) from Mexico, three European Pond Tortoises (*Emys europæa*), five Carpathian Scorpions (*Scorpio carpathicus*) from Italy, presented by Mr. T. D. G. Carmichael; a Black-backed Piping Crow (*Gymnorhina tibicen*) from Australia, deposited; two Common Squirrels (*Sciurus vulgaris*), British, two Green-horned Parrakeets (*Nymphicus uvæensis*) from the Island of Uvea, Loyalty group, purchased; a Black-backed Kaleege (*Euplocamus melanotis*) from Sikkim, received in exchange; a Hybrid Paradoxure (between *Paradoxurus larvatus* and *Paradoxurus leucomystax*), two Variegated Sheldrakes (*Tadorna variegata*), bred in the Gardens. The following insects have been exhibited in the Insect House during the past month:—Butterflies: *Papilio podalirius*, *Anthocharis cardamines*, *Araschnia levana*, *Thais polyxena*. Moths: *Deilephila euphorbiæ*, *Chærocampa elpenor*, *Sphinx pinastri*, *Saturnia pyri*, *S. carpini*. Silk Moths: *Attacus roylei*, *Actias selene*, *A. luna*, *Telea polyphemus*. The insects have, with few exceptions, been very good specimens.

OUR ASTRONOMICAL COLUMN

THE PRESENT COMET.—The following orbit of the comet discovered by Mr. Wells on March 17 has been calculated by Mr. Hind from the Harvard College and Albany observations on March 17, and observations by Prof. Tacchini at the Collegio Romano in Rome on April 6 and 21; the small corrections were taken into account:—

Perihelion passage, June 10·69852 M.T. at Greenwich.

	°	′	″	
Longitude of perihelion	53	47	46·3	From mean
,, ascending node ...	205	8	2·6	equinox
Inclination	73	57	47·2	1882·0.
Logarithm of perihelion distance	8·796420			

Motion—direct.

Hence the positions for Greenwich midnight will be—

	R.A. h.	m.	s.	Decl. °	′	Log. distance from Earth.	Sun.
May 4	21	19	11	+71	31·3	9·9870	0·0670
5	21	36	12	72	19·0		
6	21	54	57	73	1·5	9·9793	0·0502
7	22	15	27	73	37·7		
8	22	37	35	74	6·4	9·9724	0·0324
9	23	1	9	74	26·4		
10	23	25	49	74	36·9	9·9664	0·0134
11	23	51	6	74	37·1		
12	0	16	22	+74	26·4	9·9612	9·9932

If the intensity of light on March 19 be taken as unity, the intensity on May 12 is 15·6.

The perihelion distance is given by different computers as follows:—

Kreutz—observations to April	7	0·06343
Lamp ,, ,,	9	0·06123
Oppenheim ,, ,,	11	0·06459
Hind ,, ,,	21	0·06258

From the above elements it will be found that at the ascending node the comet makes a close approach to the earth's orbit, the distance being only 0·0048, or, assuming 8″·848 for the solar parallax, 443,500 miles, roughly twice the distance of the moon. The ascending node is passed on July 11, but the earth will be far from that point of her orbit.

THE SO-CALLED *Nova* of 1848.—There does not appear to be any recent notice of the magnitude of this object, though the last published observations by Dr. Julius Schmidt in 1868 showed that it had not sensibly changed for some years. It was slightly over 13m. Its position for 1880·0 is in R.A. 16h. 52m. 46·5s., N.P.D. 102° 42′ 26″. Webb in the last edition of his "Celestial Objects for Common Telescopes," p. 356, says: "colour very fine, 1875," but this note must surely refer to some other object, the *Nova Ophiuchi* of 1848, having been too faint for years past to show striking colour. Perhaps some reader of NATURE may be able to state what is its present degree of brightness. There are two stars having the following positions with reference to *Nova* which may assist its identification.

11m. ...	Angle 249°·4	Distance 7′ 55″
10·11m. ...	,, 144°·5	,, 8′ 51″

It follows a 9m. Lalande-star 14·7s., and is 18′ 22″ north of it. In 1874 it was below the twelfth magnitude.

GEOGRAPHICAL NOTES

LIEUT. DANENHAUER and two of the crew of the ill-fated *Jeannette* have arrived at St. Petersburg, where they were met with a hearty reception. Lieut. Danenhauer has little hope that Capt. De Long and those with him can have survived, though Engineer Melville is searching for them. He speaks of the unsatisfactory nature of the charts of the Lena mouths and that part of the Siberian coast, and states that Baron Nordenskjöld has added little to our knowledge in this respect. But the Baron did not profess to do so, and indeed could not, seeing that his aim was to get over the ground as quickly as possible. The Lieutenant also is not sanguine as to the possibility of opening up trade by the mouth of the Siberian rivers, forgetting apparently that the time of his arrival at the Lena mouth was past the time most favourable for navigation, and the conditions of his arrival were certainly unfortunate.

WRITING on Chinese maps, the *North China Herald* says that the present dynasty has made greater efforts at map-making than any former one, and appears to have been the first to introduce into them lines of latitude and longitude. The old maps of China are very vague and inaccurate, and are not ancient in any sense. Ssu-ma-Chien when compiling his history did not judge it needful to illustrate it with maps, but his commentators have supplied this deficiency, and recent editions of his work contain maps poorly done of China at successive periods. The geographical works of the Han dynasty do not contain maps. The first maps that have been retained in modern editions of ancient books are those of the Sung dynasty, and they seem to be connected with the invention of printing, which dates from A.D. 932. It was the influence of foreign countries that caused the Chinese to enter rigorously into the work of map-making at this period. The Buddhists began to compile works with maps of India and the countries through which lay the routes to India. One of their larger works at this time contains a map of China, of Persia and Rome, according to the geography of the Han dynasty, and a map of India as known to the Buddhists. The Mahommedans followed the latter in teaching their notions of map-making to the Chinese. But all through the Sung dynasty till the 13th century, when the Mongols established their Empire, Chinese scholars possessed but imperfect views of geography, and failed to obtain clear ideas either of foreign countries or of their own in regard to topography. During the Mongol domination many Europeans visited China and brought with them a certain portion of geographical knowledge. No steps, however, were taken by the Government to improve maps and common geographical books, which remained as bad as before. The Chinese had junks in the Indian Ocean from the 5th century, yet in the 16th century we find in maps of that time that Cambodia and Siam are islands; that Java lies west of Siam, that the Greek empire (Fulin), Arabia, and Medina are three small islands a little to the west of Java, and that an immense southern continent fringes the map from a little south of Ceylon to a point not far south of Java, and again farther east. Good maps have only existed since the Jesuit missionaries came to China, and they belong only to the present dynasty. The Emperors Khang-hi and Kien-lung encouraged the survey of their dominions and the construction of good maps. Danville's *Atlas Chinois* is the result in French of the surveys made under Khang-hi by Gerbillon and his companions. All European maps of China rest mainly on those surveys. Among the atlases of the empire, that made by a former governor of Honan province deserves special praise. It is on a large scale. Each square of 200 *li* represents a square degree. Two inches and a half represent 200 *li*. This affords ample space for names, which are freely inserted on the most frequented roads. As a specimen of engraving it is rough, and of course being on wood and done by provincial workmen it cannot equal the copperplate maps which were issued last century from the Government workshops in Peking. But it is in comparison with past times a great advantage to the people to have a map on a large scale for four or five dollars, on which both degrees and miles are marked by a system of chess-board squares with quite sufficient accuracy for ordinary use. For this they are indebted to Khang-hi and the Jesuits.

MR. C. R. MARKHAM has presented to the Geographical Society a long and careful report on the instruction at present supplied to this country in practical astronomy, navigation, route-surveying, and mapping. Although much improvement has taken place since nautical astronomy was placed in the South Kensington programme, still Mr. Markham shows that much remains to be done ere practical instruction in these important subjects is on the footing on which it ought to be in a country whose interests are so dependent on good seamanship. The Council, on the basis of Mr. Markham's report, have made a series of recommendations to the Board of Trade and the Lord-President of the Council; the former are recommended to raise their standard, and the latter to place navigation and nautical astronomy among the science subjects in the New Code. The report and the recommendations deserve serious consideration.

THE last two parts of the *Deutsche geographische Blatter* contains detailed accounts, by the Brothers Krause, of their researches in the Chukchi Peninsula, accompanied by maps and illustrations; this forms a valuable addition to the information obtained by the *Vega* Expedition. Nos. 2 and 3 of the *Mittheilungen* of the Vienna Geographical Society contains a paper by Herr Ferd. Blumentrit, on the Ancestor-Worship and Religious beliefs of the Malays of the Philippine Islands.

M. MASCART is delivering daily lectures to the naval officers who are to leave on June 1, on the Antarctic Expedition now fitting out at the expense of the French Government. These lectures are delivered at the Parc St. Maur, where instruments have been established. The lecture will be published by Gauthier Villars, after having been revised.

IN the April number of *Petermann's Mittheilungen* M. Ernest Marno gives an interesting account of the barriers of the Bahr-el-Gazal, and their removal from April to June, 1881. Dr. Fera Loïol of Prag contributes a long paper of great interest, with numerous illustrations, on the formation of terraces in the Alpine valleys. Dr. Oscar Drude writes on the botanical exploration of North Africa from Morocco to Barca.

"A VISIT to Madeira in the Winter 1880 81" is the title of two lectures by Dr. Denis Embleton, of Newcastle-on-Tyne, published by Messrs. Churchill. Dr. Embleton, besides giving his own experience, has brought together much information on the islands in all their aspects.

THE Dutch Polar Expedition, which participates in the great International undertaking, will start for Port Dickson on July 1 next. Half the cost is borne by the Dutch Government, the other half having been raised by public subscription. The expedition will return in 1884 if all is well. At the same time the annual Dutch Polar Expedition to the Novaya Zemlya region—the fifth—will start early in May from Amsterdam, commanded by Lieut. Hoffmann. They hope to return in October.

SOME OF THE DANGEROUS PROPERTIES OF DUSTS[1]

THE lecturer pointed out that the dangerous properties of dust with which he proposed to deal were altogether distinct from the subtle, invidious dangers of microscopic dust-motes which pervade the air—dangers the existence and nature of which had been fully revealed by the classical researches of Pasteur, Tyndall, &c.

Compared to those, the dangers which he would discuss were as palpable as are the comparatively gross dust-particles which give rise to them, and yet, although their existence and, to a great extent at any rate, their causes have been known and demonstrated for many years, those who are most directly interested in them and should be most keenly alive to them appear either to have ignored their serious import or to have undervalued the teachings of practical experience and scientific research regarding their causes and effects.

Seven years ago Mr. Abel, in a lecture on Accidental Explosions, delivered at the Royal Institution, directed attention to the fact that solid combustible and especially inflammable substances, if sufficiently light and finely divided to allow of their remaining for a time thickly suspended in air, may on application of sufficient flame to them while so suspended, produce explosive effects; behaving, in fact, similarly to mixtures of inflammable gases or vapours with air, with this difference, that the mobility of the molecules of these insures the ready production of complete mixtures of them with the air, so that combustion, when once established, proceeds almost instantaneously throughout such mixtures, whereas, in the case of a mixture of solid dust particles and air, the rapidity with which combustion spreads

[1] Abstract of Lecture at the Royal Institution, April 28, 1882, by Prof F. A. Abel, C.B., F.R.S.

through it depends upon the state of division of the solid, and the abundance of its distribution through the air. Under the most favourable circumstances, the rapid combustion or explosion of such a mixture is of a comparatively moderate kind, as it has to spread from one isolated particle to another. With highly inflammable solids, the rapidity of combustion under such conditions is greatest, because, as each particle burns it also evolves inflammable vapour, and is enveloped in flame which produces corresponding effects upon the immediately adjacent particles. In order to ensure rapid and complete transmission of flame through a mixture of inflammable dust and air, it is essential that the former should be present in great abundance, for the foregoing reasons, and that it should therefore be considerably in excess over the supply of oxygen in the air. The facility with which, under these conditions, flame may be transmitted by a dust-and-air-mixture, with a rapidity calculated to produce more or less violently destructive effects, according to the scale upon which the combustion occurs and the degree of confinement of the burning mixture, has been abundantly demonstrated by accidents, many of them very disastrous, which have occurred in works where large quantities of inflammable dust are unavoidably produced. Thus, in the grinding of sulphur, the inflammation of dust of that substance, consequent upon the over-heating of a shaft-bearing, has produced an explosion sufficiently violent to destroy the chambers within which the operation was conducted. In cotton mills, the accidental ignition of finely divided cotton fibre floating in the air has led to the very rapid spread of conflagrations throughout extensive buildings. Even in a factory where the spent madder, or guaracine, is ground, whereby a much less inflammable dust than that of cotton is produced, an important explosion occurred a few years ago. But the most numerous and extensive calamities of this kind have taken place, and are still of frequent occurrence, in flour and rice mills. Many such explosions, or very rapidly spreading conflagrations, occurring in different parts of the continent and here, prior to 1872, appeared enveloped in mystery, until their probable cause was indicated by an Austrian observer, and soon afterwards made clear by Dr. Watson Smith, and especially by the careful inquiry which Messrs. Rankin and Macadam instituted into the very serious and fatal explosion which occurred at the Tradeston Flour Mills, in Glasgow, in 1872. The origin of this explosion was conclusively traced to the striking of fire by a pair of millstones, through a stoppage in the feed of grain, the results being the ignition of the mixture of flour-dust and air by which the mills, inclosed in a chamber, were surrounded, and the rapid spread of flame to the mixture of dust and air which filled the conduits leading to the exhaust box, which communicated with the several other mills and with the stive-room. In this way flame was so quickly transmitted through and to various channels and confined spaces in different parts of the building as to produce violently explosive effects almost simultaneously in different parts of the buildings. Messrs. Rankin and Macadam ascertained that accidents of this nature had increased in frequency since exhaust arrangements (for collection of the fine flour) had been adopted in the more extensive mills. The precautionary measures suggested by them were, the adoption of efficient precautions for preventing the stoppage of the feed to the millstones, the exclusion of naked flames from the vicinity of these and the dust passages, and the construction of the exhaust boxes and stive-rooms as lightly as possible, and their location outside the main buildings.

The liability to the development of fire or of heat sufficient to char or inflame portions of flour by the stoppage of the feed of grain, appears from all accounts to be extremely difficult to guard against, and to have been the cause of many serious calamities even since the Tradeston explosion, examples of which are the great explosion of six mills at Minnesota in 1878, when eighteen lives were lost and much property was destroyed; and the fatel and destructive explosion of a flour mill at Macclesfield in September last, which has been made the subject of a Report to the Home Office by Mr. Richards, of the Board of Trade. It appears to be the opinion of experienced men in the trade that, although special attention to the feed arrangements may reduce the number of explosions, this cause of accident is almost impossible to guard against; while on the other hand, many fires or explosions, ascribed to it, have been due to the employment of naked lights in mills near localities where the air is laden with flour-dust. Considering that flour- and rice-mill-owners have to bear the the burden of very heavy rates of insurance, it is to their interest, independently of their responsibilities as the guardians of the lives of their workmen, to adopt most stringent regulations and efficient precautionary measures for abolishing this source of danger, and to devote their energies to the application of improved arrangements for reducing the quantity of dust which passes away from the millstones and from other parts of a flour mill.

The important part played by coal-dust, which exists in greater or less abundance in coal-mines, in aggravating and extending the injurious effects of fire-damp explosions, was originally pointed out early in 1845 by Faraday and Lyell, when they reported to the Home Secretary the results of their inquiry into an explosion which occurred at Haswell Collieries in September, 1844. That Report, which was published in the *Philosophical Magazine* for January, 1845, dealt exhaustively with the cause of the explosion, and the means by which a recurrence of such a calamity might be guarded against, and the latter subject was again discussed by Faraday in a lecture delivered at the Royal Institution in February, 1845, and in a letter published directly afterwards in the *Philosophical Magazine*. It is pointed out in Faraday and Lyell's Report, that in considering the extent of the fire from the moment of the explosion, fire-damp must not be supposed to be the only fuel, for that the coal-dust swept up by the rush of wind and flame from the floor, roof, and walls of the working would instantly take fire, and, in support of this statement, they refer to considerable deposits of dust in a partially coked condition which they found adhering to the faces of pillars, props, and walls where the explosion had occurred and the fire had extended. An examination of these deposits showed that the coal was deprived more or less completely of its bituminous constituents, and they concluded from this that the exposure of the dust to the flame of the exploding gas-mixture gave rise to the generation of much coal-gas from it, the carbon, or coke, remaining unburnt only for want of air.

Ten years after the publication of Faraday and Lyell's Report, M. de Souich, an eminent French mining engineer, published, as original, some very similar observations made by him on examining the effects of a coal-mine explosion at Firminy; he noticed, moreover, that men near the pit's mouth had received burns, while others who were in the workings near the seat of the explosion, but out of the main air current, escaped unhurt, and he ascribed this to the action of coal-dust in carrying flame along the principal air-way. Later on, De Souich extended his inquiries into the part played by coal-dust in explosions, and the subject was afterwards pursued from time to time in France by Verpilleux and other authorities in mining engineering, and especially by M. Vital in 1875, when an explosion occurred at Campagnac, the destructive effects of which appeared to him in a great measure ascribable to coal-dust. Vital made experiments upon a very small scale for the purpose of ascertaining whether flame, such as that projected into the air of a mine by the firing of a charge of powder, in a very strong blast-hole, was increased in size by the presence of suspended coal-dust; and soon afterwards Mr. W. Galloway commenced a series of experiments of a similar nature, but upon a larger scale, which he has continued from time to time up to the present date; while Messrs. Marreco and Morison, in connection with the North of England Institute of Mining Engineers, and a committee of the Chesterfield and Derby Institute of Engineers, have also contributed valuable experimental data bearing upon the influence exerted by coal-dust, not merely in increasing the magnitude of explosions resulting from the ignition of mixtures of fire damp and air, but also in propagating or even actually developing explosions, when only small quantities of fire-damp are present in the air of a mine, or where fire-damp is believed to be entirely absent. The conclusion to which Mr. Galloway was led by his earlier experiments was to the effect that coal-dust, when thickly suspended in air, had not the power to originate an explosion, or to carry on to any distance the flame from a blown-out shot, but that the presence in the air of such small quantities of fire-damp (2 per cent. and under) as an experienced miner would fail to detect by means of his Davy lamp, with which the gas is generally searched for, would impart to a mixture of coal-dust and air the property of burning and carrying flame. But he held the view at the same time, that a fire-damp explosion in one part of a mine might be propagated to some extent by coal-dust raised by the effects of the explosion in parts of the mine where no fire-damp existed. Marreco, on the other hand, considered that the results of certain experiments made in the entire absence of coal-dust, by firing shots in air travelling at some considerable velocity, and containing coal-dust thickly suspended

in it, warranted the conclusion that coal-dust also might, under certain conditions, originate an explosion as well as carry it on to some considerable extent. The results obtained by the corresponding experiments of the Chesterfield Committee appear to support this view, and Mr. Galloway has also, by his later experimental results, been led to the same conclusion, and considers that the results of his examination into the effects produced by some of the most serious of recent coal-mine explosions (at Penygraig, Risca, and Seaham) demonstrate that those explosions were chiefly, if not entirely attributable to coal-dust.

Notwithstanding the considerable light that was thrown on this subject so far back as 1845 by Faraday and Lyell, and the accumulation of experimental and other observations relating to the action and effect of coal-dust in colliery explosions, these have not until quite recently received the attention which they merit at the hands of mine owners, and many of those in authority connected with coal-mines. Evidence collected by the Royal Commission on Accidents in Mines, from mine inspectors and leading mining engineers (and published with the preliminary Report of the Commission), show the preponderance of opinion to be against the view that explosions could originate, or be to any great extent propagated, by coal-dust, in the absence of fire-damp, though the belief is entertained by many that the coal-dust may be credited with an extension or aggravation of explosions caused by fire-damp. On the other hand, there is a great tendency exhibited always to ascribe explosions which do not admit of satisfactory explanation, by an accidental failure of ventilation or other evident causes, to a sudden disengagement, or outburst, of fire-damp, such as is of no uncommon occurrence in fiery mines, and is sometimes very serious in its magnitude and duration. That such outbursts, following upon falls of roof and the firing of blast holes, have been the cause of many disastrous explosions, there can be no doubt, but, in some instances, the conclusion that an explosion had been due to this cause, is based upon assumptions and upon very doubtful evidence. Under any circumstances, it is extremely difficult to realise how sufficient gas to produce an explosive atmosphere, can be conveyed, even by the most powerful currents, from the seat of such a sudden outburst to far distant portions of the mine, to which the burning effects of an explosion have been found to extend, within the period believed to have elapsed between the first outburst of gas and the ignition of an explosive atmosphere formed in its vicinity. On the other hand, the evidence of severe burning, after an explosion, such as could not be produced by the rapid explosion of a gas-mixture alone, and the deposition of partially burned coal-dust in distinct parts of a mine, distant from each other and from the point to which the origin of the explosion has been traced, seem to leave no doubt that coal-dust has played an important part in many of the explosions which have of late been subjected to rigorous investigation.

The strong impression entertained by many, during the inquiry into the great explosion at Seaham Collieries, in September, 1880, that coal-dust might have had much to do with the accident, and that the explosion was possibly even entirely due to the ignition of coal-dust by a blown-out shot, in the absence of any fire-damp, led to Mr. Abel's being requested by the Home Secretary to make experiments with samples of dust collected in the mine, and to an extension of these experiments to dust collected from collieries in different parts of the kingdom where explosions had occurred.

The results of experiments conducted with great care and on an extensive scale at a colliery in Lancashire, where a constant supply of fire-damp was brought to the pit's mouth from a so-called blower, confirmed the fact demonstrated by M. Vital and Mr. Galloway, that the propagation of fire by coal-dust, when thickly suspended in air, is established or greatly promoted by the existence, in the air, of a proportion of fire-damp, which may be so small as to escape detection by the means ordinarily employed (such for example as exists in the return-air of a well ventilated mine).

It was also established that a mixture of fire-damp and air approaching in proportion those required to be explosive, would be ignited by a flame if only a small proportion of dust were floating in it. Further, it was demonstrated that, although those dusts which were richest in inflammable matter, and most finely divided, were the most prone to inflame and to carry on flame, in the presence of small quantities of fire-damp, some dusts which contain coal only in comparatively small proportions were as sensitive as others much richer in inflammable matter, and that even certain perfectly non-combustible dusts possessed the property of establishing the ignition of air- and gas-mixtures which, in the absence of dust, were not ignited by a naked flame. This action of non-combustible dusts appeared to be due to physical peculiarities of the finely-divided matter, and to be perhaps analogous to the contact-action well known to be possessed by platinum and some other bodies, whereby these bring about the rapid oxidation of gases which, in their absence, may exist intact in admixture with oxygen or air.

Many experiments were tried with sensitive coal-dust from Seaham and other collieries, for the purpose of ascertaining whether results could be obtained supporting the view that coal-dust, in the complete absence of fire-damp, is susceptible of originating explosions and of carrying them on indefinitely, as suggested by some observers, but, although decided evidence was obtained that coal-dust, when thickly suspended in air, will be inflamed in the immediate vicinity of a large body of flame projected into it, and will sometimes carry on the flame to some small extent, no experimental results furnished by these experiments warranted the conclusion that a coal-mine explosion could be *originated* and carried on to any considerable distance in the *complete absence* of fire-damp. Some experiments made in a large military gallery at Chatham showed that the flame of a blown-out shot of $1\frac{1}{2}$ lb. or 2 lb. of powder might extend to a maximum distance of 20 feet, while in a very narrow gallery, similar to a drift-way in a mine, the flame from corresponding charges extended to a maximum distance of 35 feet. These distances are considerably inferior to those which flame from blown-out shots has been known to extend, with destructive results, in coal-mines, and there appears no doubt that, in the latter cases, of which the lecturer gave examples, the flame was enlarged and prolonged by the dust raised by the concussion of the explosion. But, in these examples (with charges of 1 lb. of powder), the flame did not extend much beyond a distance of 100 feet, and therefore the power of the dust to carry an explosion or flame in these cases was limited. It was found, in experiments with the large Chatham gallery, in which the flame from a blown-out shot reached, in the absence of dust, to a maximum distance of 20 feet, that, when the atmosphere was thickly laden with a highly inflammable coal-dust, from Seaham Collieries, the flame was carried on to nearly double, and in one case a little more than double, the distance.

Although it may be very doubtful whether coal-dust, in the *complete absence* of fire-damp, can be credited with the production of extensive explosions, as has been recently maintained by some, there can be no question that, in the presence of only very small quantities of fire-damp, it may establish and propagate violent explosions; and that, in the case of a fire-damp explosion, the dust not only, in most instances, greatly aggravates the burning action and increases the amount of after-damp, but that it may also, by being raised and swept along by the blast of an explosion, carry the fire into workings where no fire-damp exists, and thus add considerably to the magnitude of the disaster. The supposition that extensive coal-mine explosions may be produced by coal-dust alone, in the complete absence of gas, necessitates the fulfilment of conditions which cannot but be at any rate very exceptional, but its acceptance is unnecessary to add to the formidable character of coal-dust as a source of danger and an agent of destruction in mines. The possibility of dealing with the dangerous dust in mines should therefore be as much an object of earnest work as has been the improvement of ventilating arrangements in mines.

The actual removal of dust-accumulations being in most instances impracticable, the laying of the dust by an efficient system of watering the mine ways, is a matter deserving serious attention. Although in some instances such a measure is not readily applicable, without injury to the workings, it has been already proved in some districts to be unobjectionable and susceptible of very beneficial application. The employment of deliquescent substances (calcium chloride, sea-salt, &c.), in conjunction with watering, has also been advocated and tried to some extent with success.

The elaboration of really safe and sufficient methods of getting coal where blasting by powder is now resorted to, and of removing the harder rock in the working of drifts, &c., where fire-damp may exist, must most importantly contribute towards the diminution of danger arising from the accumulation of dust in mines, both by avoiding the projection of flame into the air, and by avoiding powerful concussions, whereby dust is raised; and the lecturer referred in conclusion to the various plans, in

addition to coal-cutting machines, which had been devised to dispense with powder, or render its employment safe. The use of compressed air had been attended by some measure of success, and the dispersion of water, used as tamping, by the explosion of a powder charge in the form of a spray, had been shown to have frequently, though not reliably, the effect of drowning the flame developed by the explosion. The employment of water-columns, by which the force developed by the detonation of dynamite was uniformly transmitted throughout the entire length of the hole, had been proved, by experiments in coal-mines in Lancashire, and special test-experiments at Cardiff, to render that material very suitable for coal-getting, and at the same time to render blasting possible without liberation of flame. Lastly, the employment of cylinders or cartridges of compressed quicklime, according to a simple system elaborated by Professors Smith and Moore, was referred to as ranking before all other methods of getting coal, yet proposed, in point of simplicity, cost, and above all, safety, and the lecturer described operations witnessed by himself with this system of coal-getting at Shipley Collieries. In concluding, Mr. Abel exhorted those interested in, or entrusted with the working of coal-mines, to spare no pains to test rigorously and fairly the merits of any processes or methods of affording promise of dispensing with the employment of powder in the ordinary way, and thus of securing protection to the miner against combined dangers of fire-damp and dust.

THE INFLUENCE OF TEMPERATURE ON CERTAIN SEEDS

ON regarding seeds of our hardy trees which are sown in autumn, and which do not germinate before the return of spring, we feel forced to admit that however the other conditions may vary, the cause which causes the germination in the commencement of the fine weather is the rise in the temperature, and one is equally tempted to think that the higher the temperature, as long as this rise does not equal that which would destroy the seeds, the more active and pronounced would be the germination. Nevertheless this is not by any means always the case, at any rate in the seeds of the walnut and almond trees. Anxious to germinate some of these seeds in winter, Prof. H. Baillon thought to obtain a more rapid development in a warm house, in which the temperature varied within the twenty-four hours from 15° to 25° (59 – 77 F.), than in a cool house in which during the same time the temperature varied between 5° and 15° (41 – 59 F.), but the trial proved a failure. In the cool-house, in the course of six weeks, the walnuts had stems of about two decimetres in height, whereas the most advanced of those in the warm house had only stems of from two to three centimetres in the fully developed leaves. The experiment was several times repeated. The same quality of earth, and the same quantity of water was used, and the results were the same. After a space of two and a half months the greater part of the nuts sown in the warm house had only roots occasionally well developed, but little or no caulome outside the fruit. Moreover, the greater part of the walnuts which germinated in a house, where there was good bottom-heat, had roots which did not behave like those of walnuts, germinating in the cool house and without bottom heat, the tap root of the latter grew well in length before any egress of the plumule, whereas the tap-roots of those grown in the warm house were early arrested in their development, and this through growing in a very friable soil, consisting of moist sawdust, much less resisting than the sand or the earth of the cool-house, in which the tap-roots developed so well. This was very nearly the same with the almonds, and would seem to point to the fact that in the case of some seeds there is no advantage to be gained by forcing them. Some, like *Eranthis hiemalis*, at whatever period they are sown in the open air, will develop themselves at a fixed time, as it does in January (H. Baillon in No. 39 of the *Bulletin Periodique de la Soc Linn. de Paris*, January, 1882.)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

PROF. HENRY ALLEYNE NICHOLSON has been appointed to the chair of Natural History in Aberdeen, vacant by the removal of Prof. Cossar Ewart to Edinburgh.

DR. SORBY, F.R.S., has been elected president of Firth College, Sheffield.

THE Nottingham University College Committee have appointed Mr. Wm. Garnett, of St. John's College, Cambridge, to the Professorship of Mathematics and Physics, at the College, vacant by the resignation of Prof. Fleming.

SCIENTIFIC SERIALS

American Journal of Science, April.—The wings of pterodactyles, by O. C. Marsh.—Sandstones having the grains in part quartz crystals, by A. A. Young.—Notes on American earthquakes, No. 11, by C. J. Rockwood.—Notes on the electromagnetic theory of light, No. 1, by J. W. Gibbs.—The "timber line," by H. Gannett.—Simple method for calibrating thermometers, by S. W. Holman.—Notice of Fisher's "Physics of the Earth's Crust," by C. Dutton.—Physiological optics, No. III., by W. L. Stevens.—Great dyke of Foyaite or Elæolite-syenite in North-Western New Jersey, by B. K. Emerson.—Notice of the remarkable marine fauna occupying the outer bank off the southern coast of New England, No. 51, by A. E. Verrill.—Determination of phosphorus in iron, by J. L. Smith.

Journal de Physique, March.—On the electro-chemical equivalent of water, by M. Mascart.—Studies on the psychrometer, by M. Angot.—Electric Lighting (concluded), by M. Foussereau. Determination of the ventral segments of sonorous tubes by means of manometric flames, by M. Hurion.—Compensator for measuring electromotive forces, by M. Slouguinoff.—On phosphorographs of the solar spectrum, by M. Becquerel.

April.—On a simple law relative to natural magnetic double circular refraction, by M. Cornu.—Determination of the illuminating power of simple radiations, by MM. Crova and Lagarde.—Measurement of potentials corresponding to determinate explosive distances, by M. Baille.—Study on the combustion of explosive gaseous mixtures, by MM. Mallard and Le Chatelier.—New dry sensitive thermometer, by M. Michelson.

Sitzungsberichte der physkalisch-medicinischen Societät zu Erlangen, 13 Heft, November, 1880, to August, 1881.—On the action of the milk-juice of *Ficus carica*, by A. Hansen.—On the artificial production of double-formations in chickens, by L. Gerlach.—On intra-thoracic pressure, by J. Rosenthal.—On the law of dispersion, by E. Lommel.—A polarisation apparatus from platinocyanide of magnesium, by the same.—The germinal plates of Planaria, by E. Selenka.—Contributions to the theory of binary forms, by M. Noether.—Observations on the composition and exchange of material of the electrical organ in the torpedo, by T. Weyl.—On a new way of permanently fixing small anatomical objects for the purposes of demonstration, and preserving them without use of alcohol, by L. Gerlach.—On the compression of drugs, by J. Rosenthal.—On the influence of chemical agents on the amount of assimilation of green plants, by T. Weyl.

Rivista Scientifico-Industriale e Giornale del Naturalista, January 31.—Mode of rendering the Holtz machine more active, by C. Marangoni.—The radiometer and school experiments, by C. Rovelli.—On a Querquedula new to Italy, by A. Fiori.—New applications of the pneumatic method for rapid desiccation of large Orthoptera, &c., by P. Stefanelli.—Preparation of Hemiptera, by G. Cavanna.—Contribution to the study of anthropology of the Southern Provinces, by M. del Lupo.

February 28.—Nephoscope of P. F. Cecchi.—On the synthesis of various organic acids, by Drs. Bartoli and Papasogli, through electrolysis of water and of acid on alkaline, &c., solutions with carbon-electrodes, by P. Guasti.—Differential apparatus for determining the ozone in air, by D. Tommasi.—Observations on the habits and the development of *Æschna cyanea*, Müll., by P. Stefanelli.

March 15.—On *Lebia turcica*, Fab., by F. Piccioli.—Lombard palæontology; fossil fauna of Lombardy, by A. Stoppani.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. xv. fasc. iv.—On some fossil insects of Lombardy, by F. Sordelli.—Some theorems on the degenerate forms of ellipsoid of Culmann, by G. Jung.—The double quadratic transformation of space (continued), by F. Aschieri.—Geometrical construction of the universal transformation of the third order, by E. Bertini.

Fasc. v.—Reduction of integrals of algebraic functions to integrals of rational functions, by C. Formenti.—What are the most simple and sure means of radical cure of hemorrhoidal varices? by A. Scarenzio.—Aberrations of the sexual sentiment

in a gynæcomastic maniac, by A. Raggi.—On varied systems of forces, by G. Bardelli.

Fasc. vi.—Origin of the *Tractus olfactorius* and structure of the olfactory lobes of man and of other mammalia, by C. Golgi.—Some theorems on the development in series by analytic functions, by S. Pincherle.

Atti della R. Academia dei Lincei, vol. vi., fasc. 7.—On the tombs and dwellings of Iberian families existing in Italy, by L. Pigorini.

Natura, March.—A heat-electrometer, by G. Gandini.—On the origin of electricity of thunderclouds (concluded), by F. G. Nachs.—Alpine meteorology, by P. F. Denza.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, April 29.—Anniversary Meeting.—Prof. W. H. Flower, F.R.S., president, in the chair.—After some preliminary business the report of the council on the proceedings of the Society during the past year was read by Mr. Sclater, the secretary. It stated that the number of fellows on December 31, 1881, was 3213, against 3309 at the same time of the previous year. The total receipts for 1881 had amounted to 25,810*l*., against 27,388*l*. for 1880. The ordinary expenditure for 1881 had been 24,651*l*., against 24,753*l*. for 1880; and the extraordinary expenditure 1036*l*., against 1825*l*. for the preceding year. The sum of 1000*l*. had been devoted to the repayment of the mortgage debt on the Society's freehold premises, which had thus been reduced to 6000*l*. This expenditure had left a balance at the bankers of 1203*l*. to be carried forward for the benefit of the present year. The assets of the Society on December 31 last, after payment of all liabilities, were estimated to amount to nearly 20,000*l*., exclusive of the value of the library and collection of living animals. As regards the gardens in the Regent's Park, little has been done in the way of special works during 1881, but the buildings and walks had been kept in good repair, and several of the former had been thoroughly repaired and painted. The number of visitors to the Society's Gardens in 1881 had been 648,604, against 675,979 in 1880. The zoological lectures having been well attended during the past year, would be continued during the present season. The number of animals in the Society's collection on December 31 last was 2294, of which 617 were mammals, 1389 birds, and 258 reptiles. Eleven mammals, 17 birds, and 11 reptiles belonging to species new to the collection had been exhibited in 1881, and during the same year a considerable number of mammals, birds, and reptiles of a different species (of which detailed lists were given) had reproduced their kind in the Society's Gardens.—It was moved by Viscount Powerscourt, seconded by Mr. Howard Saunders, that the best thanks of the meeting be given to the Council for their report. The motion having been adopted, the meeting proceeded to elect the new Members of the Council and the officers for the ensuing year, and a formal ballot having been taken, it was declared that Mr. H. E. Dresser, Prof. Mivart, F.R.S., Mr. Henry Pollock, Mr. W. Ayshford Sanford, and Capt. George E. Shelley had been elected Members of the Council in place of the retiring Members; that Mr. F. Du Cane Godman had been re-elected into the Council in the place of Mr. Edward R. Alston, deceased; and that Prof. Flower, LL.D., F.R.S., had been re-elected president, Mr. Charles Drummond treasurer, and Mr. Philip Lutley Sclater, M.A., Ph.D., F.R.S., secretary to the Society until the next anniversary.

Physical Society, April 22.—Prof. Clifton, president, in the chair.—New member, Dr. E. Hopkinson.—The president announced that copies of the Report of the Lightning Rod Committee could be obtained from Dr. Guthrie, Science Schools, South Kensington, price five shillings per copy.—A paper was then read by Mr. W. F. Stanley on the evidence of a flowing liquid moving by rolling contact upon the interior surface of a pipe. In his experimental work on fluids, published last year, the author has endeavoured to show that liquids flowing in a tube move by rolling contact on or past the resistent surfaces of solids, and upon like principles that the moving parts of a flowing liquid move by rolling contact on the more quiescent parts of its own mass, so that in no case is there any element of sliding, gliding, or shearing motion such as is generally assumed. Further experiments tend to support this view in the case of liquids flowing through pipes. The difficulty in the experiments arose from the friction of the pipe impeding the free motion of the particles. The principle was investigated by allowing liquids o various kinds, such as solution of mastic varnish, to flow through pipes, the liquids containing colouring matter, or air particles to assist the eye. The author illustrated the effects by diagrams on the screw.—Dr. W. H. Stone, Mr. Blaikley, Dr. Guthrie, and the President, offered some remarks on the paper.—Mr. J. M. Whipple exhibited the magnetograph curves obtained at the Kew Observatory during the past week, showing the progress of the recent magnetic storms. After stating that two unusually large spots were now passing over the sun's disc, he remarked that although the magnets at Kew were somewhat disturbed on the 14th, they were nearly stationary until the night of the 16th, when, about 11.45 p.m., they became strongly affected, and from then till 8 p.m. on the 17th, the magnetic storm raged. The horizontal component of the earth's magnetic force was at one time reduced more than 0.05 mm. mgrs. below its average value, and the vertical component by about 0.07 of the same units. This happened about 6 a.m. of the 17th. A little after noon of the same day both forces became so increased, that the light spot left the scale of the instrument for nearly two hours. A second period of magnetic disturbance commenced at about 3.40 a.m. of the 20th, and was violent up to 2 p.m., subsiding gradually until 7.45 p.m. of the 21st. During this period, the magnetic force, though fluctuating largely, did not experience such great changes of intensity as were indicated by that of the 17th. Mr. Whipple then alluded to the work of Prof. W. G. Adams, and suggested that sun-spots only produced such effects when cutting certain lines of force, which he imagined might extend for a limited angular distance round the earth's radius vector. Prof. Adams pointed out the desirability of increasing the number of self-recording magnetic observatories, especially in the southern hemisphere, and after mentioning that the French were about to equip such an observatory at Cape Horn, expressed the wish that the Cape of Good Hope Observatory might again be provided with magnetometers.—The Rev. S. J. Perry remarked on the exceptional nature of the storm which he had seen recorded at Brussels, and stated that in Belgium the telegraph service had been disorganised by it. Attention was also called to the auroral displays in America, and Mr. Lecky, Dr. Guthrie, the President, and others, spoke on the general phenomena of the storms.—It was then announced that the meetings of the Society in May would be held on the 6th and 20th, instead of on the 13th and 27th, as previously announced; also that the Society would hold a meeting at the Clarendon Laboratory, Oxford, on June 17, by invitation of the president.

Victoria (Philosophical) Institute, May 1.—Prof. Reinsch gave the results of his researches into the mode of the formation of coal. The lecture was illustrated by several large drawings and photographs. The professor stated that he had examined with the microscope no less than 2500 sections of coal, and had come to the conclusion that coal had not been formed by the alteration of accumulated land plants, but that it consisted of microscopical organic forms of a lower order of protoplasm, and although he carefully examined the cells and other remains of plants of a higher order, he computed that they have contributed only a fraction of the matter of coal veins, however numerous they may be in some instances, he referred to the fact that Dr. Muck, of Bochum, held that algæ have mainly contributed to the formation of coal, and that marine plants were rarely found in coal, because of their tendency to decompose, and that calcareous remains of mollusks disappeared, on account of the rapid formatiou of carbonic acid during the process of carbonic action.

Royal Horticultural Society, March 28.—Sir J. D. Hooker, in the chair.—*Savagus floccosus:* Mr. Pascoe exhibited specimens of this beetle from Queensland, attached apparently by a species of *Isaria*, while living.—*Doryanthes Palmeri:* Sir J. D. Hooker exhibited a leaf, some five feet long, and a cluster of flowers from a spike twelve feet in length, bearing a fanicle of flowers, eighteen inches in length.—*Coryanthes macrantha*, exhibited by S. T. Laurence; the fertilisation of which, by insects, is described by Mr. Darwin, in his "Fertilisation of Orchids."

EDINBURGH

Royal Socety, April 17.—The Rev. Dr. Lindsay Alexander, vice-president, in the chair.—Prof. Blackie communicated a paper on the definite article in Greek, with special reference to the revised version of the New Testament. He showed by quo-

tations from classical Greek authors, that the Greeks were anything but particular in the use of the article, and apparently attached little importance to it; and hence a slavish rendering of the article when it occurred into English, not only led in many cases to bad English, but displayed ignorance of true scholarship. This fault, the author maintained, the revisers had in not a few instances made their own.—Prof. Blyth, in a paper on the action of the microphone, pointed out that the action due to the aërial waves of sound directly, and that due to the tremor of the frame-work which supported the microphone, must be carefully distinguished, the latter being probably the source of the jarring that so commonly accompanies telephonic sounds. As the result of a series of ingeniously-contrived experiments, he concluded that the true microphonic action, as far as it related to the transmission of articulate sounds, is due to the direct action of air-pulses upon the temporary minute "arc-lights" which exist as soon as the carbon points are shaken asunder by the tremor of the frame.—Prof. Marshall submitted an account of experiments made by Prof. C. Michie Smith, Mr. R. T. Omond and himself, in reference to the lowering of the maximum density point of water by pressure. The lowering was measured indirectly by calculation from observed thermal effects when the pressure in a mass of water at a given temperature was suddenly diminished from several tons' weight on the square inch to the atmospheric pressure. This thermal effect is a heating effect when the temperature of the water is below that of the maximum density, a cooling effect when above. A thermoelectrical junction let into the pressure apparatus, and connected to a delicate galvanometer, noted the changes of temperature, while the pressure was measured by means of one of Prof. Tait's high-pressure gauges, formerly described. From their first and preliminary series of experiments, they had deduced a lowering of the maximum density point of water by 2° C. per ton's weight increase of pressure.

Paris

Academy of Sciences, April 24.—M. Jamin in the chair.—The following papers were read:—Movements of various parts of a liquid in a vessel or reservoir, whence it flows through an orifice (continued), by M. de Saint Venant.—Researches on the distribution of heat in the dark region of solar spectra, by M. Desains. With a rock-salt prism, he found the position of the cold bands and the maxima always nearly the same. But there was not the same agreement in the relative values of the intensities of successive maxima and minima (especially in the region of great wave lengths). The maxima were much greater in 1882 than in 1879 (doubtless owing to dryness of the air).—Memoir on the temperature of the air at the surface of the ground, and of the earth to 36 m. depth, also of two pieces of ground, one bare, the other turf-covered, in 1881, by MM. Becquerel. The mean air-temperature, 11°·15, is higher than in the two immediately preceding years.—On quarantines at Suez, by M. de Lesseps.—Separation of gallium, by M. Lecoq de Boisbaudran. He uses advantageously cupric hydrate, instead of carbonate of baryta or lime, for precipitation of galline; the copper is easily eliminated with sulphuretted hydrogen.—Report on a memoir relating to the hygienic and economical properties of maize, by M. Fua. M. Fua proves the value of maize as food; maladies attributed to it have been really due to badly kept and diseased maize.—On hypercycles, by M. Laguerre.—On the theory of uniform functions of a variable, by M. Mittag-Leffler.—On Fuchsian functions, by M. Poincaré.—Solution of the general problem of indeterminate analysis of the first degree, by M. Méray.—The minima of sun-spots in 1881, by M. Ricco. The northern hemisphere was observed (at Palermo) without spots, 23 days; the southern, 94 days. There were 12 periods of minima in the north, and 18 in the south. The intervals of minima differ little from the time of a solar rotation. A certain stability of minima thus indicated, especially in the northern hemisphere, was confirmed by observations of longitude.—On the actinic transformation of Foucault mirrors and their applications in photography, by M. de Chardonnet. A plate of rock crystal, silvered so as to be opaque to sight, forms a filter, permeable exclusively by dark rays of short wave-length, and which may be used for photography without intervention of visible light. Very white crown glass or thin Saint Gobain glass may be used instead of rock salt.—On magnetic perturbation, by M. Mascart. A magnetic storm of large extent seems to have begun (after some preliminary indications) on the night of April 13, and continued a week or more; strong shocks occurred on the 16th and 20th.—Winter of 1881-1882 at Clermont and at Puy-de-Dôme, by M. Alluard. These stations showed interversion of temperature with altitude on 78 nights in four months (November to February), or nearly two-thirds; the minima on Puy-de-Dôme ranging from 7 to 13 degrees above those at Clermont. New proof was had of the rule that whenever an area of high pressures covers central Europe, and especially France, the interversion in question occurs. M. Faye, referring to the fact that Mont Blanc is sometimes seen from Puy-de-Dôme, distant 280 km., suggested observation of geodesic refraction between them.—On the equivalent of carbon determined by combustion of the diamond, by Prof. Roscoe. Representing O by 15·46, C becomes 11·07.—On the decomposition of salts of lead by alkalies, by M. Ditte.—Action of sulphuretted hydrogen on solution of sulphate of nickel in the cold state, by M. Baubigny.—Researches on ozone, by M. Maillefert. He describes its action on sulphur, selenium, tellurium, sulphides, and some organic matters.—On the absorption of volatile bodies with the aid of heat, by M. Schlœsing. He illustrates this by several experiments; *e.g.* powders of carbonate of ammonia pass into a small tower of coke, sprinkled with dilute sulphuric acid; a part of the alkali is carried beyond. If the temperature be raised to 100°, the absorption is total, and almost instantaneous. M. Schlœsing proposes to apply the principle to determination of nitric acid in the atmosphere.—On the oxidation of pyrogallic acid in an acid medium, by MM. Clermont and Chautard.—On the insoluble modification of pepsine, by M. Gautier.—On nuclei with intense polychroism in dark mica, by M. Lévy. These are due to zircon.—On the action of permanganate of potash against accidents from the poison of Bothrops, by M. Couty. His conclusions, from experiment, are adverse to use of the permanganate as antidote.—On the abyssal malacological fauna of the Mediterranean, by M. Fischer. From 555 m. to 2660 m., about 120 species of mollusca were dredged, of which only about 30 are really abyssal. The number of species diminishes sensibly with the depth. All the deep species are also found in the Atlantic.—On some attempts at hybridation between different species of Echinoideæ, by M. Kœhlen. These were successful, *e.g.* in the case of a Spatangus and a Psammechinus.—On some points of the anatomy of Holothurians, by M. Jourdan.—On the pyloric ampullæ of Podophthalmate crustaceans, by M. Mocquard.—On the vitality of trichinæ encysted in salt meat, by M. Fourment. In salt meat prepared fifteen months back were live trichinæ, which were fully evolved in the alimentary canal of a new host, and caused death.

CONTENTS

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-Dôme
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Blanc is
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by Prof.
-On the
-Action
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by M.
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